PHILOSOPHICAL SOCIETY

THE
QUARTERLY JOURNAL,

October, 1821.

Art. I.—*On an ancient Monument of Sculpture in Stone, representing the Theban Sphinx; which was recently discovered at Colchester, in Essex. By* E. W. A. Hay, Esq., A.B., F.A.S.*

THERE is not perhaps any single object of ancient art, that has been ever found in this country, which offers so much interest, not merely to the antiquary, but to the artist and to the historian, as this very extraordinary and beautiful sculpture lately discovered.

The engravings (Fig. 1, 2, 3, and 4†,) are after drawings made, as nearly as the perspective would admit, according to a scale of one quarter the size of the original. They represent the four principal views of the Sphinx, which the sculptor has exhibited as having already slain some victim of her wiles; her bloodthirsty passion would seem to be already satiated; she sits as it were satisfied with her prowess, and in perfect serenity, over the mangled remains of her unsuccessful opponent.

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* In a letter which the writer of this article published lately at Colchester, upon the same subject, he announced his intention of transmitting the four drawings, here engraved, to the Society of Antiquaries of London, for the purpose of insertion in their Archæological publication. The drawings have been exhibited to the society, and excited considerable interest with that learned body; but, as the forthcoming volume of the Archæologia was already completed, and in the press, and there was not any probability of part of another being published within a twelvemonth from the present time, it was thought desirable to convey to the public, some knowledge of this curious discovery, through the circulation of this Quarterly Journal.

† The figures 1 and 2 present the right and left views of the monument: the figures 3 and 4, shewing the front and rear views, will be given in the following Number of this *Journal*.

This singular figure, which, from its beauty, might naturally be expected to be of the finest marble, has been sculptured in freestone. The material appears, from its good quality, to have been brought from the Isle of Portland; since it is not known that any stone of the same kind has been found in a native state in the neighbourhood of Colchester. The Romans brought, no doubt, from Portland, much of the materials for their finer works at that place where they planted their first colony[*] in Britain, and which long remained one of their principal military stations.

This Sphinx was found in the midst of a great number of fragments of another species of stone, equally unknown at Colchester as a native product, and vulgarly called *Swanage*, from the place where it is dug in the Isle of Purbeck, which is in the immediate vicinity of Portland.

The general dimensions of our antique are as follows:

Length of the base, ...	25½	Inches.
Medium breadth of the base, ...	10	
Height from base to top of Sphinx's head, .	25	
The face of the Sphinx, measuring from under the chin to the crown of the head, . .	5	

The stone was found at the depth of about two feet from the surface of the soil, in trenching the ground around the General Hospital lately erected at Colchester. It was discovered in an almost perfect state, there being only a few marks of injury, and those slight; excepting upon the left side of the work that lay uppermost. The fracture of the nose of the Sphinx, as well as a blow upon the man's forehead, were, with some other less material bruises, the almost unavoidable effects of the labour upon discovery.

But the circumstances of the spot where this relic has been found, as well as the great beauty and the peculiar character of the work, have stimulated inquiry, and merit some discussion.

Notwithstanding all that has been written upon the early history of Colchester, it may be useful, as a preliminary step to our consideration of the probable history of the monument in question, to refer those who retain any hesitation upon affixing

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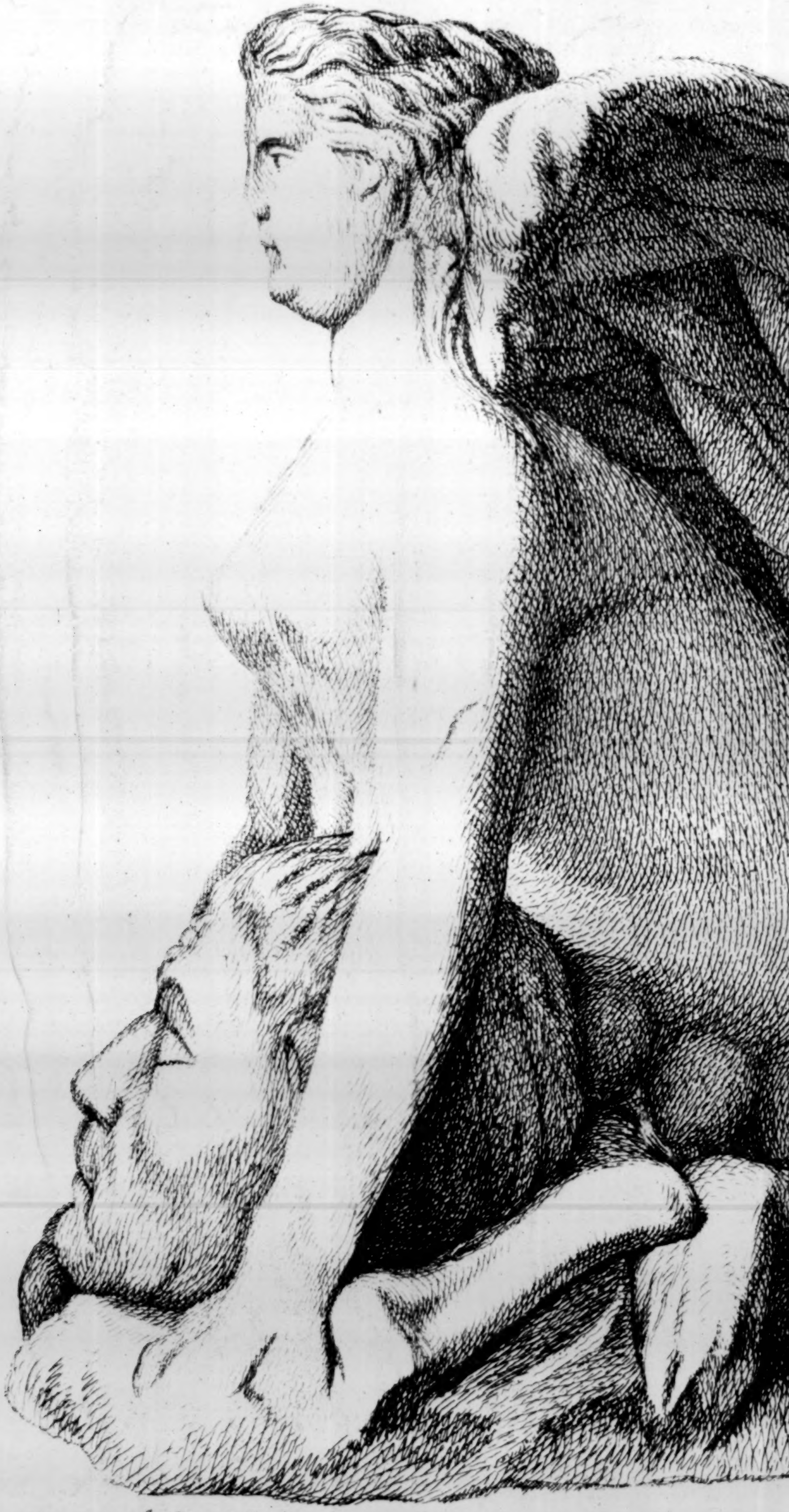
[*] Tacitus *Annal.*, L. 14, and Camden upon Essex.



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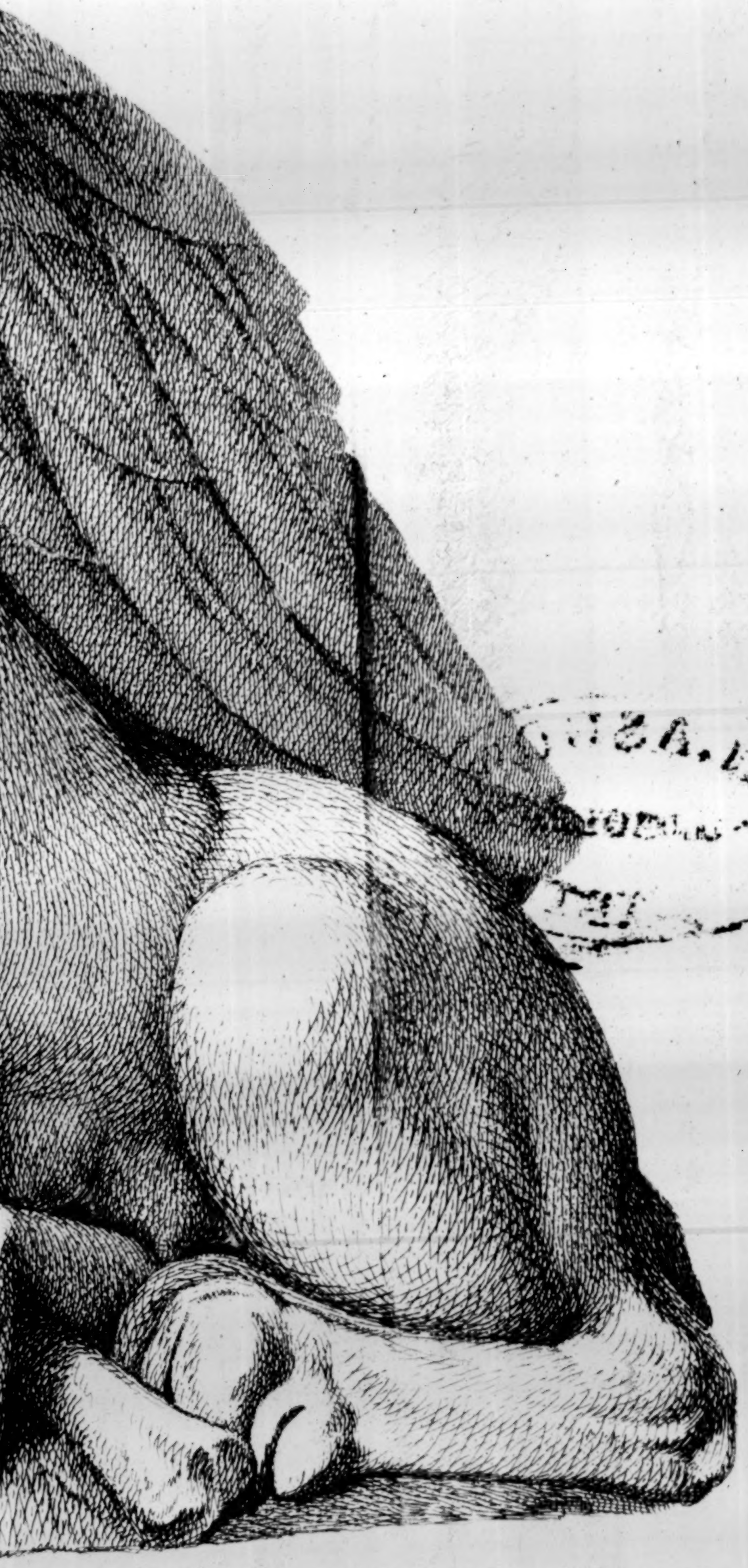

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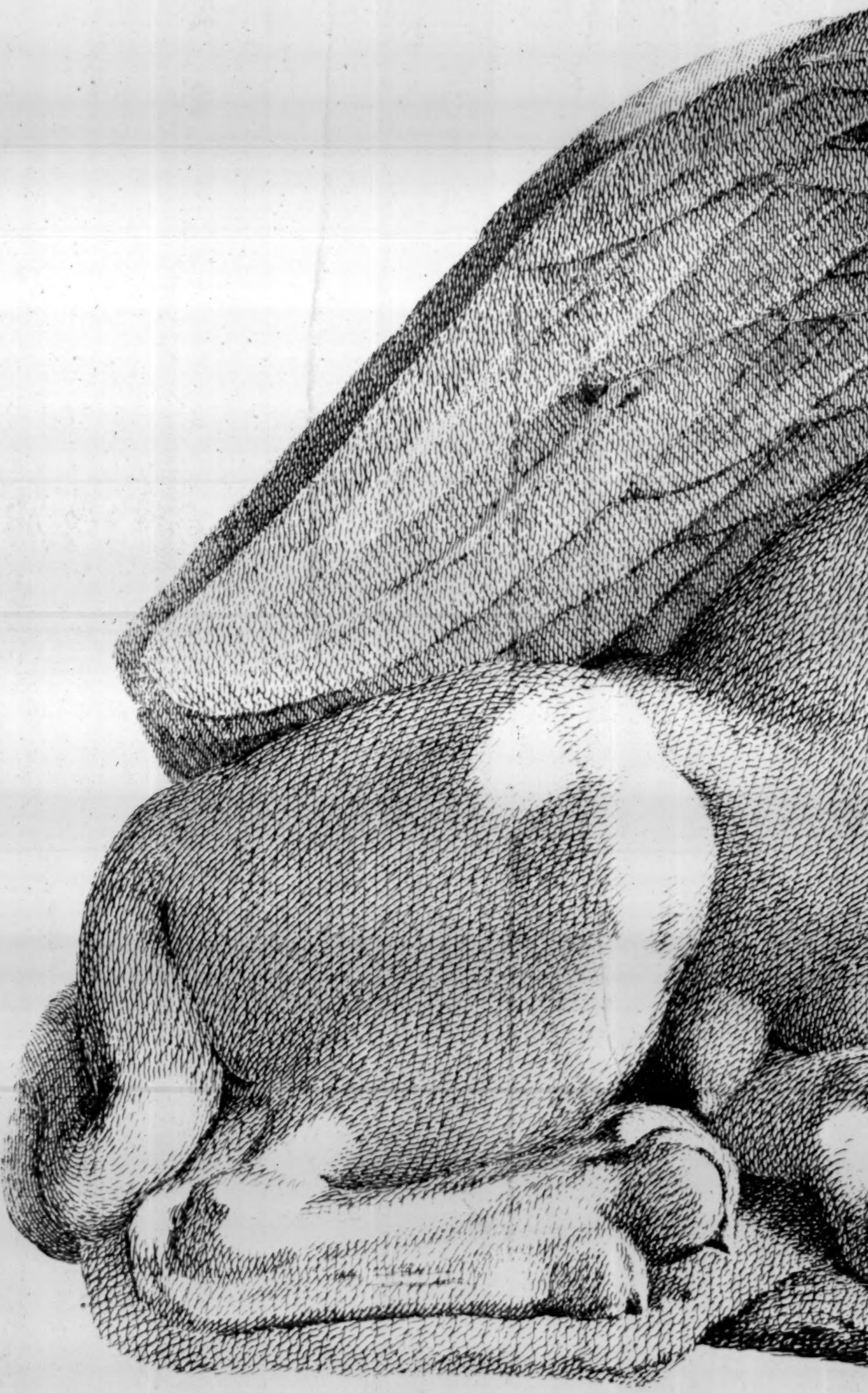

Ancient Sphinx discovered

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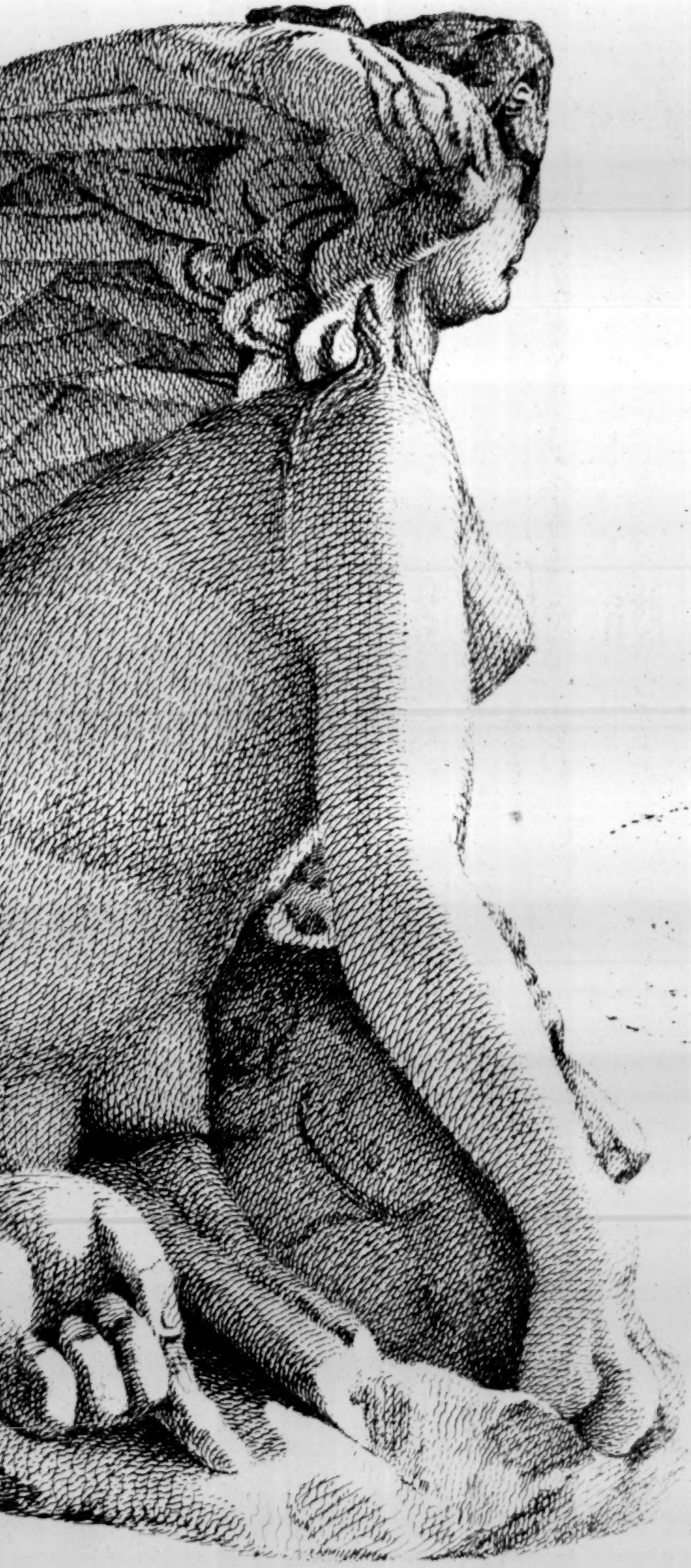

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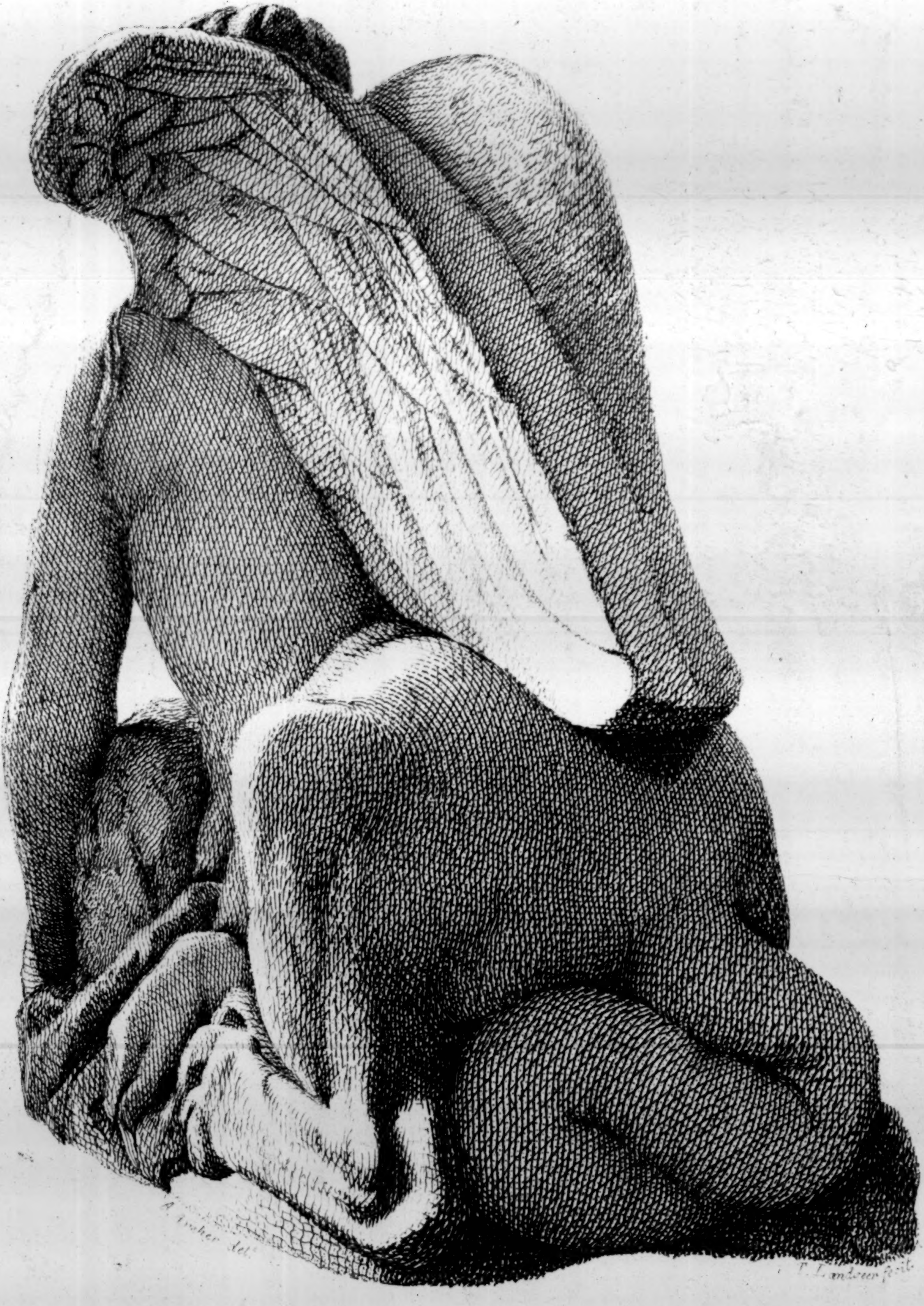
Fig. 4.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Ancient Sphynx discovered at Colchester

See note at Page 1.

London, Published Jan.1.1822 by John Murray, Albemarle Street.

the *Colonia Camulodunum* of the Romans at Colchester, to the Archæologia. Mr. Walford, adducing several authorities upon whose judgment and learning the most scrupulous may be willing to rely, has, in the 16th volume of that publication, concluded an interesting dissertation upon the situation of Camulodunum in these terms: "I hope the preceding observations, which from my own personal inspection I have found very accurate, will be the means of preventing any future controversies or difference in opinion upon the subject; and that all will agree with Bishop Stillingfleet, Dr. Stukeley, Dr. Mason, Mr. Morant, Mr. Gough, and the Rev. Mr. Leman, that Colchester, and not Malden, was the *Colonia Camulodunum* of Richard of Cirencester*."

Having premised thus much, I proceed to present such hypotheses upon the probable age, authors, and history of the monument under review, as may be fairly deduced from a consideration of its own character, of various remains of antiquity found near it, and of the historical records immediately applicable to the place of discovery.

The adjustment of the hair of the Sphinx, is the same as that of the younger empress Faustina; yet it resembles almost as closely the dress that we have frequent occasion to notice upon Roman monuments of an earlier time. But when we consider the evidence of the sculpture itself, whose chaste, yet cultivated character is as far distant from the rude operations of the earliest British workmen, as it is from the meretricious style of any middle time in this country; we need not hesitate to ascribe our Sphinx to the chisel of some eminent Roman sculptor, and of the best age.

The coarse nature of the material militates against the probability of this fine piece of work having been imported from Italy. We may therefore be allowed to conclude, that it was wrought in this island; for it is acknowledged that the Romans brought artists hither with them, and they may also have been invited over by the British Princes †.

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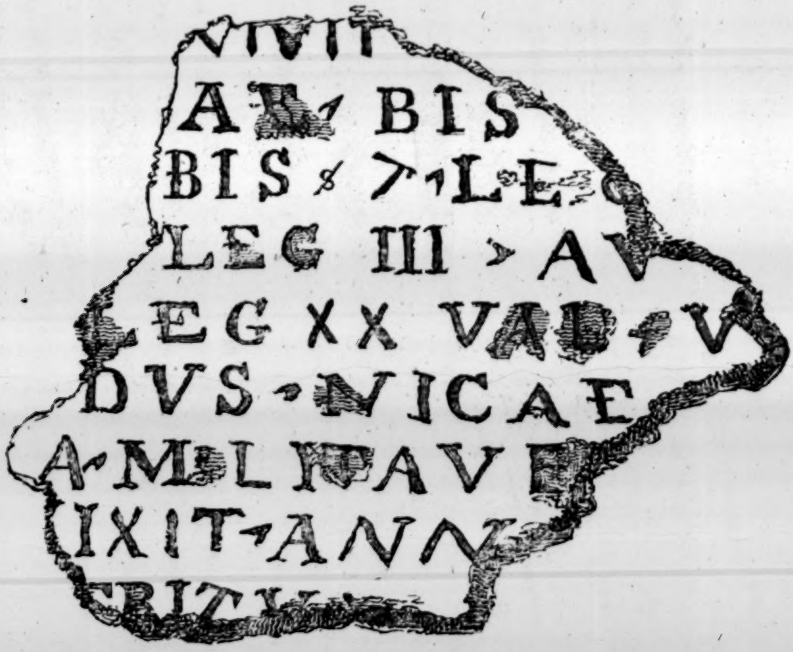
* See Pegge *on the Coins of Cunobelin*, and Ruding's *Annals of the Coinage.*

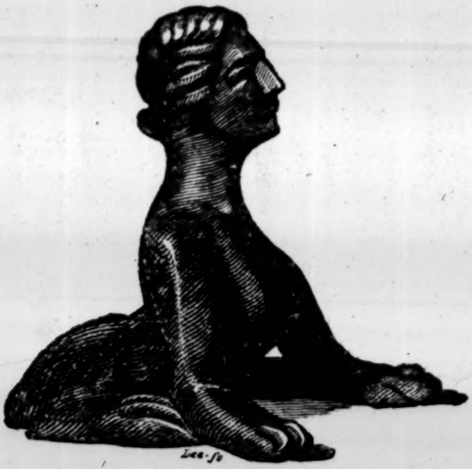
† See Pegge *on the Coins of Cunobelin*, p. 54, &c.

Of the Roman origin of this monument, we have irresistible proof in the great mass, found in and around the very spot where the Sphinx was discovered, of antique remains: such as bricks, tiles, pottery, fragments of bronze, and other relics, that are decidedly of Roman fabric; and which, as well as the Sphinx, lay concealed in a soil that apparently had not for centuries been disturbed beyond the depth of the plough-share. But my attention has been particularly attracted by the portion of a sepulchral inscription, to the memory of one or more legionary Roman soldiers: this was dug up a few days sooner than the Sphinx, and at the distance of no more than about twenty-five paces from it. The following cut is a faithful copy, although the engraver has formed the letters with less sharpness and care, and represented them as being more mutilated, than the state of the original will justify.



This fragment is moreover of itself extremely curious, as making a distinct mention of the *Third* Imperial Legion, (LEGionis III. AVgustæ) a portion of the Roman forces, of whose appearance in Britain I cannot find any trace by all the references I have been able to make. The stone has 10½ inches by 8 of surface, and is 1⅜ of an inch thick. Let it be noticed, that this fragment is of the stone brought from Swanage; and it is further an extraordinary relic, as, excepting one other fragment of a sepulchral inscription, which is of the same material, but cut with greater delicacy, and found at Colchester about twelve years ago *, no Roman remains of a similar kind are known to have been discovered either at Colchester, or even within the county of Essex. This, from the long residence of the Romans in that quarter of the island, is indeed almost unaccountable; but may be judged of by the void it has occasioned in Horsley's *Britannia Romana* †.

We have now to offer an unexpected testimony in favour, not only of the Roman origin of our stone Sphinx, but also of the peculiar reverence in which, perhaps, that very same beautiful figure of sacred legend, or at least the more common and less complicate mystic symbol, was held at Camulodunum; by the fortunate finding of a bronze Sphinx, of which the following engraving presents a view, the same in size as its original ‡.



The bronze is perfect, excepting the loss of the wings, which, from the appearance of the back, have evidently been torn off. This, which I deem to have been one of the *lares* of an inhabitant, was dug up last summer within a few yards of the spot, where our stone Sphinx was discovered. The little image exhibits, in its present state, no further compound than of the lion and the virgin; and, from the arrangement of the hair resembling that of Julia Mæsa, or of her daughter Julia Sœmias (mother of the Emperor Heliogabalus), as well as from its inferiority of style and execution, it is doubtless of a later time than the large Sphinx; not appearing to claim any earlier date than about the beginning of the third century of our era.

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* Now in my possession.

† Since this article went to the press, John Disney, Esq., of the Hyde, near Ingatestone in Essex, has been so obliging as to make known to me a small sepulchral monument in his possession, which was found at Colchester in 1713, and bears the following inscription. It is a tablet of marble, eight inches long, by four and three-quarters high, having a border of foliage and other work in an inferior style:

CONSIDIA·VENERIA·FILIA·V·A·III·D·XXX·CONSIDIA·NATALIS·MATER·V·A·XXXV.

‡ In the possession of a lady at Colchester.

But, although I do not feel myself at liberty to refer the age of our stone monument to an earlier time than that of the first Emperor Claudius, as will be hereafter shewn; yet I consider that we are fully authorized in tracing the respect for the Sphinx symbol, that would appear to have been entertained by the inhabitants of *Colonia Camulodunum*, as well by the indigenous Briton, as by the Roman colonist, to a higher origin, even to the time of Augustus. We learn from Pliny, from Suetonius, and from Dion Cassius, that the image of a sphinx was, during a certain period of the life of Augustus, employed by that emperor as his seal. We find also the figure of the sphinx upon the Roman coins of * his time: but what comes more directly

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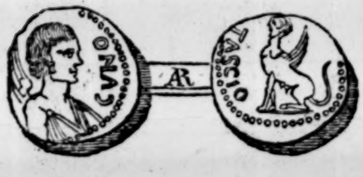

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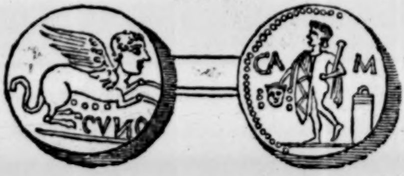

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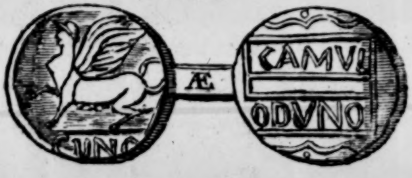
No. 3.



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* Upon the passage referred to in Suetonius, Lævinus Torrentius has the following comment: " (De Sphinge) et eodem fortassis referendum quod T. Carisii, qui sub Augusto III. vir monetalis fuit, numismata sphingem præferunt." See also Morelli and Illustrations by Agostini in *Reg. Imp. Rom. Numism.* No. 17. tab. 12.

† No. 1. " Obv. CVNO. The bust of a winged figure, possibly a Victory. Rev. TASCIO. A Sphinx, from a coin of Augustus.—No. 2. Obv. CVNO. A Sphinx. Rev. CAM. Qu. A British warrior with the head of an enemy in his right hand?" See British Coins, plate 4 and 5, Nos. 8 and 25 in Ruding's *Annals*.—No. 3. Obv. CVNO. A Sphinx. Rev. CAMVLODVNO.

in aid of our argument is, that we meet with the same figure repeatedly upon the coins of Cunobelin.

This illustrious British prince * had, according to Dion Cassius, his royal seat at Camulodunum; and more of his coins are found at and near to Colchester, than in any other part of the island. Of the three here given, Nos. 1 and 2 have been already published; No. 3 is now, I believe, for the first time, presented to the public: the original is in the possession of Mr. William Keymer, of Colchester, where it was found about twenty-five years ago. I possess also a small brass coin of Cunobelin, found at the same place in 1819: it is in a bad state of preservation, but bears upon both sides a very striking resemblance to No. 3; yet mine appears to have had the wheel under the right paw of the sphinx, as we find exhibited in Gem, No. 206 of Agostini, in No. 111 of Tassie, and elsewhere. This addition No. 3, from the action of its sphinx, may have also had, but it seems to have suffered corrosion upon that part.

Cunobelin is said to have cherished a friendship for the Romans, to have attended Augustus in his wars, and so well to have behaved himself, that he grew into particular favour with that emperor, and accompanied him to Rome, where he was saluted by the title of Friend to the Republic: moreover, that during his residence there, Tenuant (his father, and immediate predecessor on the Trinobantine throne,) paid no tribute †.

The attachment for the Roman emperor, at least the politic respect which our great British chief ‡ might have entertained, or merely put forth, in regard to his powerful protector, may be in some measure estimated by the appearance of the head of Augustus impressed upon the coins of Cunobelin §; who, as Mr. Pegge writes, "omitted no opportunity of making his court to that emperor." We find also some notices in Suetonius, of the continued friendship that appears to have subsisted between

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* Dion L. 60.

† See *History of Colchester* by Morant; who in this matter, does in common with other respectable writers, give credit to Geoffrey of Monmouth.

‡ Or as Baxter styles him, the *Pendragon*. *Gloss. Vo. Cunobelinus.*

§ Pegge on his *Fifth Class*, No. 1. Also Ruding's *Annals*, v. 1, p. 268, an v. 5, pp. 13—17.

the Iceni and Trinobantes and the Romans; even from the time of Julius Cæsar to that of the first Claudius.

Morant thus epitomizes from Tacitus, from Dion Cassius, and others, the victorious invasion by the latter emperor, and the events which immediately followed: "Claudius took Camulodunum, where he placed a colony of veterans. In honour of this victory he was divers times saluted *Imperator*, contrary to the Roman custom, which permitted it but once in one expedition. After this he ordered the Britons to be disarmed; but to those that yielded, he remitted the confiscation of their goods, which so endeared him to them, *that they erected a temple and an altar to him at Camulodunum*, and honoured him as a god."

This is the only Roman temple of which we have any account as having been erected at Colchester; and that building I apprehend to have stood upon the very spot where the stone Sphinx was discovered. The situation is peculiarly striking, as it accords better than any other that can be chosen in or around the town, with the probable position of that sacred edifice. This will readily be acceded, when its elevated situation be taken into view, with its neighbourhood to the grand military way, and the presentation under such an aspect of its hallowed fane to all those in intercourse between the great camp at Lexden * and the capital of the Trinobantes. Since some may be unwilling to give credit to my position as taken in reference to the Roman way, objecting that such roads were not constructed so early as the reign of Claudius; I beg to observe, that, although these roads may not have been extended generally throughout Britain so soon as the first century of our æra, yet there must, without doubt, have been, from the first establishment of the Roman conquest, a grand public street leading from so principal a camp as that of Lexden (where such extensive vestiges still remain) to the great Roman colony established at the Trinobantine metropolis of Camulodunum. To others again who may remark, that the spot where I would fix the temple, is without the ancient walls, I beg to recall the words of Tacitus †, by whom it is shewn, that no walls existed around the town at the time of this temple. Indeed, had there been such a circuit of defence, we should bear in mind, that it was by no means uncommon with the Romans to construct temples without their towns; and they seem, with all other nations of antiquity, to have preferred high places for such holy purpose.

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* Baxter, *Vo. Camulodunum.* † *Annals*, L. 14, ch. 31.

My conjecture is singularly supported by the very great mass of building materials, that have been thrown up during the late foundation of the General Hospital. It must have been evident to all those who witnessed, as I did, the recent disturbance of the soil upon the identical spot under consideration, that some very large building had stood on the same ground; but, at an early period, of which no local tradition now remains.

I have already pointed at the Roman character, of the antiquities discovered upon the spot: to which I might add the coins of that people, which are found there from the time of Julius Cæsar to that of the Constantines.

Considering the pains that have been taken at Colchester, for many successive centuries, to remove from their foundations all remains of Roman edifices convenient in the erection of the buildings, which succeeded at every period; the quantity of stones and Roman bricks dug out from the present hospital garden did appear extraordinary. I observed not only red bricks and tiles of undoubted Roman manufacture, but have also remarked, at the same place, several specimens of a costlier species of white tile, evidently the fabric of that people. Amidst a large quantity of unhewn stone, which has been lately thrown out, and was probably used in foundation, and other works equally removed from the eye, there have been discovered many and well hewn fragments of Swanage; much of which stone is observable also in the castle, and among other Roman materials in the walls of the town, in the churches, and in other ancient buildings at Colchester.

While the workmen were digging last year for the foundation of the hospital, I remarked continually the bones of oxen, deer, pigs *, and fowls, amongst the Roman remains: and, from

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* These were evidently of the wild species.

similar observations that have occurred upon the sites of other Roman temples, it is far from being unreasonable to consider these bones as relics of sacrifice.

We know that it was a custom with the Romans, as with the Greeks and the Egyptians, to place images of the Sphinx in the *pronaos* of their temples. It is precisely to such a purpose, that I suspect our beautiful sculpture in stone to have been dedicated, in the vestibule of the temple of Claudius *.

But it may be asked, Why, in such a situation, should a mutilated victim be exhibited under the fangs of the Theban Sphinx †? I cannot certainly venture in an unhesitating tone to attempt an explanation of this peculiarity. Yet, if we adopt the views of Hoffmann, this exhibition of the utter destruction of the victim may be fairly accounted for, and may be shewn to have been perfectly adapted to the supposed situation of this Sphinx, as a warning emblem for all who presumed to pass the holy precincts, to enter even the porch of the temple. Hoffmann ‡ considers the fable of the Sphinx, destroying such as did not understand her mysteries, to intimate that those who observed not the precepts of the gods, were abandoned to her, as to the infernal minister of that divine wrath, which would not fail to consign the disobedient to torments and to death §.

In reply to any hesitation that may be felt in granting the claims advanced for the Roman origin of our antique stone, upon the ground of the victim being thus introduced; I beg leave to observe, that it is only requisite to turn to Montfaucon [*], to Hoffmann [†], to Raspe [‡], or to Millin [§], among the mass of antiquarian authority to which we might refer, for the exhibition of a victim conjoined with the image of the Grecian Sphinx. Yet, in the careful attachment, which is sometimes perhaps too exclusively retained, for precedent; we may be called upon to give an example from antiquity, of the destruction of a victim of the Sphinx, being shewn as already completed. The following view of an Amethyst Gem, copied from the *Traité des Pierres Gravées* of Mariette [||], is for this purpose very satisfactory.

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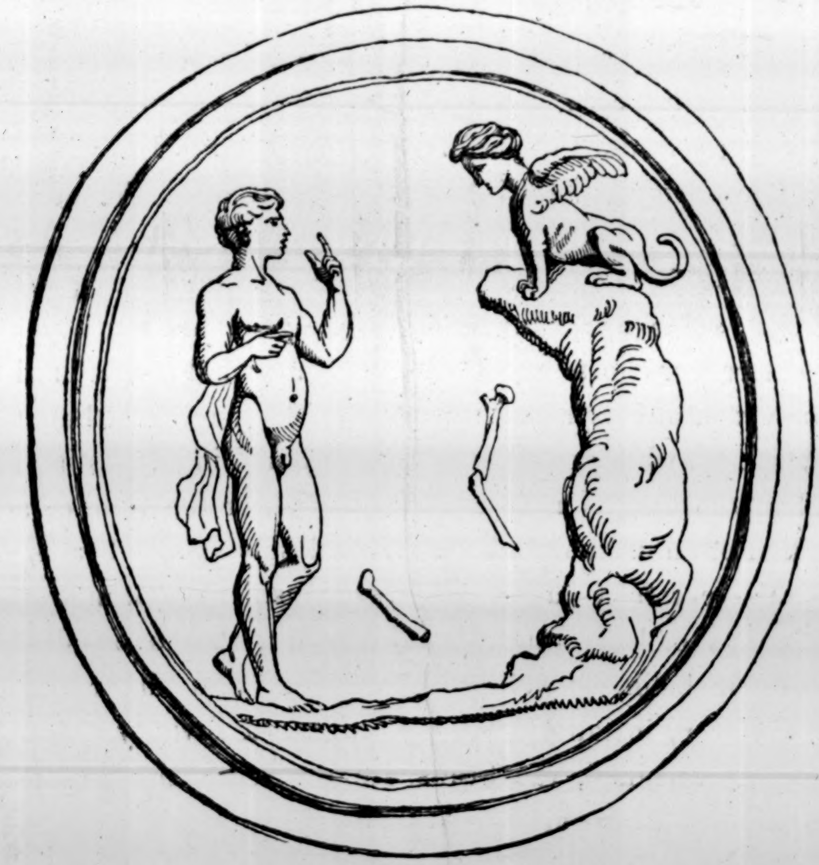
* In the same temple, it is supposed that the worship of *Camulus* was also maintained. This may have been a native appellation of the God Mars, thus latinized, as it was in many other parts of Europe; where the worship of the Deity under that designation, would, from various and authentic records, appear to have been observed.

† See Pausanias, Bœot. c. 26. The base of our figure appears to have been left in a rude state; this may be allusive to the rock, upon which the oracular monster was said to have her residence; and whence she rushed to destroy those who could not solve the riddles she propounded.

‡ *Lexicon*, Vo. Sphinx.

§ Pausanias, describing the celebrated temple and statue of the Olympian Jupiter, says—" Before his feet the Theban youth are seen forced away by Sphinxes; and under the Sphinxes, Apollo and Diana are piercing with their arrows the children of Niobe. B. 5, ch. ii.—Mr. Taylor's translation.



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[*] *Antiq. Expliq.* T. 2, part. II. c. xvii. [†] *Lexicon*, Vo. Sphinx.

[‡] *Descriptive Catalogue of Tassie's Gems*, No. 8596, *&c.*

[§] *Galerie Mytholog.* Pl. 142, No. 502.

[||] I here beg leave to express my thanks to the learned medallist in the British Museum, Taylor Combe, Esq.; for his obliging courtesy in having pointed out this Gem, as well as several coins of Cunobelin, which were before unknown to me.

We have here Œdipus expounding the enigma of the Sphinx *, who is seated on a rock; whence she would seem to have thrown, as if in defiance, under the hero's eye, the bones of some one who had unhappily preceded him. Again, we find at No. 106 of *Tassie's Gems*, a fine engraving upon carnelian †, which Mr. Raspe has classed amongst the Egyptian antiques, having a Sphinx winged and sitting, with *a death's head at her feet.* No. 8,601 of the same collection, is taken from an ancient engraving upon onyx ‡, which represents "Œdipus explaining the enigma of the Sphinx, who is sitting upon the top of a rock," Here also may, as I think, be discerned *a human scull at the bottom of the rock* §.

But what shall we say upon that very extraordinary reverse of the preceding coin, No. 2 of Cunobelin; representing, as Mr. Ruding questions doubtfully, "a British warrior, with the head of an enemy in his right hand?" and upon the obverse is a Sphinx!

I have elsewhere observed, that the head of the man upon the Colchester stone has many portrait-like peculiarities; that the hair is moreover short, appearing as if artificially curled; that although it would represent a middle-aged man, yet that it is beardless: all which circumstances combined in suggesting my first idea, that it had not been modelled after the head of any individual of an ordinary class. The closely-shaven beard, as well as the carefully-curled hair, were fashions, not only with the Romans of high rank, but, I remark, that upon all the coins of Cunobelin, as well as upon those of an earlier British stamp, in every instance, excepting where a divinity seems to have been personified, the heads are dressed with short hair; where the state of the art could attain the expression, it appears curled, and the faces are always beardless. Mr. Pegge has also noticed, that although, according to Cæsar *, the Britons wore their hair long, but shaved it on every part of the body, except the head and upper lip, yet that there are no signs of the flowing hair upon the coins: the reason for which he apprehends to have been, that their princes were exempt from this general rule.

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* According to the vulgar tale of the Sphinx's riddle, as proposed to her by Œdipus, whose solution of it, is said to have been the cause of her destruction; it was: What animal is it, that in the morning walks on four legs, at noon on two, and at night on three? To which the Hero answered—"Man."

† *Cab. Flor. Gori Mus. Flor.* II. No. 91, 2.

‡ Stosch MSS. Cat. 41, 7.

§ The sulphur impression that I have of No. 8,601 is not very distinct.

I am inclined to differ from Mr. Ruding, in his description of the above coin, No. 2, and to consider that, from the costume of the human figure, it is meant for the image of some Roman divinity: this idea receives support from the altar near to it, as well as from the reverse of another coin of Cunobelin, given by Mr. Ruding in his Appendix †. Scarcely can the reverse of the coin, No. 2, be thought to represent the effigy of a Roman emperor; much less a priest, or any one at sacrifice, the back being turned to the altar.

But, before any attempt at a conclusion, I refer again to the British coins, engraved in Mr. Ruding's work. Plate ii. No. 22. Rev. " Probably a Briton driving his chariot over a falling enemy." Nos. 23, 24, 26, and 53, following, appear to represent the same thing; and in Nos. 28 and 29, the hand underneath the (horse or other) four-footed animal with a human head, and raised, as it were, in a supplicating posture, seems to be a similar indication. In all these examples, (as well as in some other coins of the same class), the quadruped has the head of a man, or rather of a woman; and in Nos. 23, 24, and 53, it is distinctly winged. Further, upon the head of many of the same monsters, we may perceive a cap or coif, resembling that upon the head of the Sphinx, in the coin No. 3, here engraved.

Thus, in the absence of every chance for obtaining from history, any lights by which to penetrate this dark matter; we can only attempt to guide ourselves by such interpretation, as may not be thought too extravagant in an apposition of those monuments of antiquity, that are supposed to be remains of nearly the same time in this country.

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* *De Bell. Gall.* L. 5.

† Append. to the Annals. Pl. 29, No. 6.

In a former publication upon the Colchester Sphinx, I had hazarded a supposition, that the head of the mutilated victim might have been designed, in conjunction with the more ancient emblem of Grecian allegory, to allude to the united wisdom and strength of the Roman army, having in co-operation with a native Trinobantine force, succeeded in the destruction of the power of some great chief of another district of the island.

Yet, further than mere conjecture, I dare not adventure: here therefore I dismiss this abstruse subject, with these rude sketches of theory perhaps presumptuous. I may have already incurred the charge, of wildly expatiating upon what time has placed, as I fear, far beyond the ken of the most learned inquirer. Indeed could we see through all the darksome distance of its history, the amplest results might prove, like many of the fruits of antiquarian labour, to have been scarcely worthy our ungrateful toil.

There now remains little more, for our immediate purpose, than to point out the probable circumstances that may have preserved unto our time, this Stone Sphinx; which I apprehend to have been a principal decoration of the temple erected at Camulodunum, in the time of the first Claudius.

This Sphinx had probably been placed upon a pedestal, and perhaps companion to another; as we see in Montfaucon, that a Sphinx was erected upon either side the entrance to the temple of Diana Pergæa. It has evidently been designed, to be seen at an elevation somewhat above the level of the eye; for the only parts of the work, which have not been highly finished, excepting around the base, are the upper edges of the wings at their contact.

The temple at Camulodunum is recorded by Tacitus to have been destroyed by the natives, irritated with the tyrannous sway of the Romans at that station*. The historian says expressly, that this temple which had been erected in honour of the deified Claudius, was looked upon by the inhabitants as a fortress, built for the purpose of their eternal bondage; and that the priests, appointed, as in appearance, for objects only of religious service, wasted the substance of the people. The soldiery, having no proper citadel at the time of the British irruption into their settlement, fled for shelter within the walls of the temple; there they endeavoured, but in vain, to defend themselves for two days, when it was taken by storm. The temple would appear to have been destroyed with the rest of the colonial buildings, which were burnt or razed to the ground by the exasperated Britons. The Sphinx fell, as we may suppose, in the undiscriminating ruin; and although the statues of Claudius, or of the Roman Mars himself might be objects so hateful, as to merit the more particular enmity of Boadicea and her people; yet the less prominent magnitude of our relic might escape the observation of the wild destroyers; and might have lain concealed for centuries, and would probably have still remained so for as many ages more, had not the most unexpected accident brought it to light, from amidst the confused variety of ruin whence I so lately assisted in disinterring it.

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* A. D. 62.

The engravings here presented, are sufficiently satisfactory for a general view of the sculpture; and, as a contemplation of the original can alone enable one to form a correct idea of its beauties in detail, I shall subjoin only a few words, with respect to the chief characteristics of the Colchester Sphinx.

It comprises every component part of the allegoric monster of classic fable. The stone presents us with the bust of a young maiden of graceful delicacy, conjoined in a gradual transition, that is admitted by the intermediate form of the body of a bitch, with the ponderous yet evidently active powers of a lion's hinder quarter. This portion of the symbolic figure, is completed by that most expressive emblem the serpent, which forms the tail; and it is introduced with considerable skill in tortuous foldings, that keep it, without undue constraint, within the narrow limits, that the confined size of the material has imposed upon the artist. The back of the Sphinx is furnished with the wings of an eagle, beautifully raised, though not expanded. Thus, our various emblem, which may perhaps, without offence to history, be styled a "Gentile Cherub," in the language of a late writer upon Pagan Idolatry[*]; or again, a Symbol of the Great Universal Mother of Mythology; or in other terms, (to adhere to expressions of more common use), this Pantheic Sign combines in a striking manner the principal attributes apportioned to their host of gods, by the fancies of the heathen: we will not wait here to inquire, whether these several images were the offspring of grateful love, or of the more effective impulse of fear, in the original simplicity of very early times; whether they were altogether, or in part, a corrupted copying, or, as some deem them, a perverse depravation, of sacred record; or otherwise the mere result of hieroglyphic painting and poetic fancy. These mystic signs, thus arbitrarily adapted and conjoined, are now brought before us into one view, by the first antique copy of Grecian art of this occult character, that has ever been discovered in Britain[†].

With respect to the origin of the *Theban Sphinx*, of which so many varied tales have been promulgated, and of whose name so many etymons have been advanced; it may be sufficient here to observe, that the Grecian Sphinx is, no doubt, immediately derived from that of Egypt. The Egyptian Sphinx has hitherto been more commonly acknowledged the prototype of all the others; as supposed to be derived from a simple emblematic reference to the annual inundations of the Egyptian valley, by the waters that descend from the hills of Abyssinia, and overflow the banks of the Nile, while the sun is passing through the signs of *Leo* and of *Virgo*[‡]. I have not however yet found that any one has combated with sufficient evidence, an hypothesis that I have recently proffered, of this fabulous form having been taken by the Egyptians, from the Chaldaic or Assyrian Sphinxes. The Mithraic figures of ancient Persia, that call forth the wonder of modern travellers, among the wreck of Persepolis, are doubtless cognate with, or directly copied from, those of Babylon, which have from time to time been discovered amid the ruins of that *most ancient city;* and are in some instances *found there of precisely the same form as the Egyptian Sphinx**.

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[*] See *the Origin of Pagan Idolatry*, by G. S. Faber, B.D., for much curious speculation on this subject.

[†] I should not omit noticing that, under the very roughly-hewn base of the Sphinx, is clearly engraven a large and well-formed Roman S, of somewhat more than five inches in height. It appears from its situation (removed altogether from the eye of the spectator) to indicate, not so much the artist's name, or any circumstance intended to be recorded; as simply the intention of the inferior workman, who having been employed to prepare the block for the sculptor, thus marked it as a particular stone selected and set apart by the latter, for the representation of the Sphinx.

[‡] Such an application of the Sphinx figure by the Egyptians, would

But wandering amid the mazes of conjecture, vainly endeavouring to obtain a clear idea of this antique matter, the reader may be looking anxiously forward to the end of our almost hopeless inquiry; and may well be disposed to exclaim with the outwearied patience of *Milphio* in the play:

> Si nequeo facere, ut abeas, egomet abiero :
> Nam isti quidem hercle orationi Oedipo
> Conjectore opus est, qui Sphyngi interpres fuit.
>
> *Plautus—Pœnulus,* Act. 1.

Art. II. *Some additional Facts relating to the Division of the Eighth Pair of Nerves, communicated in a Letter to the Editor of the Journal of the Royal Institution, by* A. P. W. Philip, M.D., F.R.S. Ed., *&c.*

I cannot, in any way, refer to the division of the eighth pair of nerves, without taking the opportunity to acknowledge the very candid and liberal manner in which Mr. Broughton

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appear still more likely; if the etymon of its name be allowed (according to the Abbé Pluche) to be found in the Hebrew, signifying nothing more than *Superabundance.* "*Sphang*, redundantia, Job xxii. 11. and 4 Reg. ix. 17. and Paraph. Chaldaic. in Proverb. iii, 10. 'Vino torcularia redundabant.'"—*History of the Heavens*, v. 1. b. 1. c. 1.

* I allude more particularly to a very ancient Gem in fine preservation, which, with several others of extreme curiosity, has been found within these few years, upon the Site of Babylon, by Captain Lockett, who brought them to this country, and with a sight of which I have very recently been favoured by Mr. Landseer, to whom they are intrusted.—See *Archæologia*, Vol. xviii., Art. 45.

has, in the last Number of the above Journal, stated the results of the experiments lately performed at the Royal Institution.

It appears, from the observations of that gentleman, that it is admitted by all who witnessed these experiments, that after the eighth pair of nerves are divided in the neck, and the lower portion folded back, little, if any, progress is made in the digestive process; whereas, if the divided ends are not displaced, its progress is still considerable.

An interesting question here arises, whether in the latter case the nervous influence is still conveyed in consequence of the divided ends of the nerves remaining in actual contact, or in consequence of its passing through moisture or other intervening bodies.

In order to determine this question, the following experiment was made, in which Mr. Cutler, Assistant Surgeon to the second regiment of Life Guards, had the goodness to assist me.

A rabbit was kept without food for twenty-four hours, and then allowed to eat as much parsley as it chose; as soon as it ceased to eat, the eighth pair of nerves were divided in the neck, without displacing them. It was evident, both to Mr. Cutler and myself, that at the moment the division of the nerves was made, the ends so retracted as to cause them to separate from each other to about the distance of the sixth part of an inch. The animal was allowed no food after the operation. It was found dead at the end of eight hours.

On the stomach being opened, it appeared that digestion of the new food had made considerable progress. The neck was examined, and the divided ends of the nerves were found to have remained at the above-mentioned distance from each other.

This experiment was repeated in all respects in the same manner. The ends of the nerves now retracted to the distance of a quarter of an inch from each other on both sides. The animal lived about six hours, and the digestion of the new food was far advanced.

Mr. Brodie examined the state of the food and the position of the divided nerves in this rabbit, and both he and Mr. Cutler, allow me to state, that they are satisfied that the nervous influence had, in the foregoing experiments, passed from the upper to the lower portions of the divided nerves.

The following facts, relating to the division of the eighth pair of nerves and the power of galvanism in obviating its effects, appear now to be admitted on all hands,—

That after these nerves are divided in the neck, and the lower portions folded back, little, if any, progress is made in the digestive process.

That if the lower portions, thus folded back, be connected with the positive end of a voltaic battery of a certain power, the other end of the battery being connected with the skin over the stomach, the efforts to vomit which follow the division of the nerves do not take place, and digestion goes on as perfectly, as far as can be judged from the appearance of the contents of the stomach, as in the healthy animal.

That when, instead of the lower portions being folded back, they, as well as the upper portions, are allowed as much as possible to remain in the natural position; digestion makes great progress, notwithstanding the divided ends so retract as to be separated from each other by the distance of a quarter of an inch.

That the difficulty of breathing occasioned by the division of the eighth pair of nerves in the neck is prevented by the influence of the voltaic battery applied in the way above pointed out.

The reader will judge how far these facts tend to establish the identity of the nervous influence and galvanism.

But in judging of this question, it is necessary in addition to the foregoing facts, to keep in view those which seem to prove that the nervous influence is altogether distinct from, and has nothing in common with, the sensorial and vital powers, and possesses no properties but those of a chemical agent; and that the influence of the voltaic battery is not only the best of all stimuli to the muscular fibre, and capable of passing along the nerves in either direction, but is also capable of raising the temperature of living arterious blood, while it can produce no such effect either on venous blood, namely, that which has already undergone the effects of the secreting power, or on arterious blood, which has lost the vital principle. I need not here refer to the well-known facts of electricity being evidently conveyed by the nerves and under the power of the will in certain animals, and the newly dead brain forming, with other parts of animals, a galvanic apparatus.

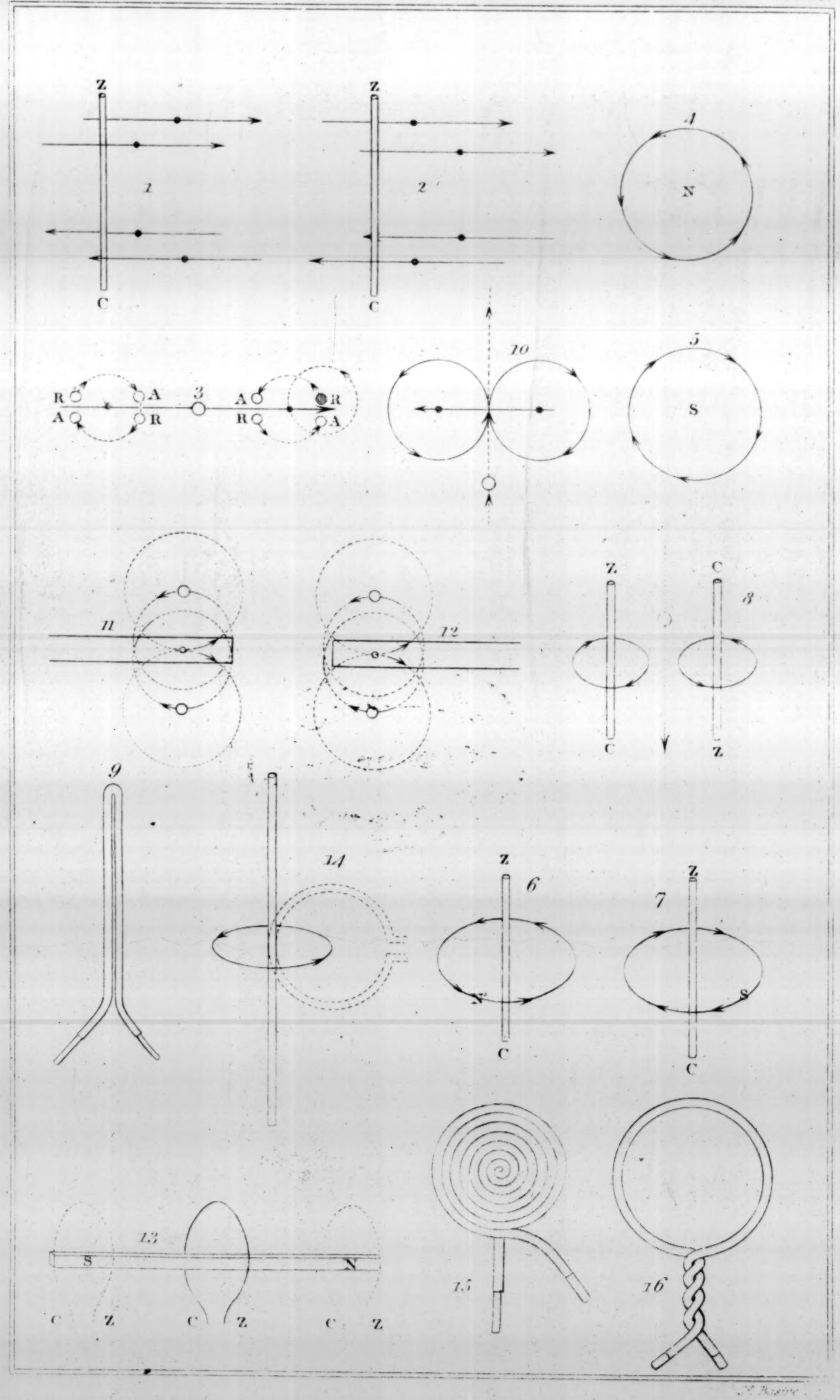
It has been said that the restored digestion and free breathing produced by the influence of the voltaic battery after the interruption of the nervous influence, may arise from the former acting as a stimulus to the secreting surfaces; but it will appear, I think, that this explanation which is derived from the laws of the muscular system, is founded on a false analogy. It is proved, by the most simple experiments, that after the nerves of a muscle have been divided, the application of a stimulus still calls it into action; the cause of which is explained by those experiments which prove that the muscular power is independent of the nervous system, and only affected by its influence in the same way as by other stimuli. But with respect to the secreting power, all the experiments on the subject combine to prove that it so immediately depends on the nervous system, that it ceases to exist as soon as the influence of that system is withdrawn.

It may be proper to add, that the experiment which proves that the division of the nerves only destroys the secreting power when one portion of them is folded back, is an *experimentum crucis* respecting the subject of my late discussion with Dr. Alison. It appears from it, that it is not the injury done to the nerve, but the interruption of the nervous influence, which destroys the secreting power.

Mr. Field found that an opening made in the trachea of a horse, large enough freely to admit of the introduction of two fingers, had no effect in relieving the dyspnœa occasioned by division of the eighth pair of nerves.

GLASGOW
PHILOSOPHICAL
SOCIETY.

Plate 3. *Vol 12.*



J. Basire.

Published by John Murray, Albemarle Street, [illegible]

ART. III. *On Secret Writing, in reply to* Mr. Chenevix's *Challenge. By the* Rev. Edward Hincks, A.M., *and formerly Fellow of Trinity College, Dublin.*

THE expedients which have been adopted by persons who have occasion to carry on a correspondence necessary to be concealed, and yet liable to be intercepted, have been very numerous. More than one might be pointed out, by which absolute inscrutability is attained with a very trifling expenditure of trouble: but as our regularly-bred physicians refuse to administer the nostrum of a quack, even in diseases where its healing efficacy is acknowledged by every one but themselves; so professional cipherers have always exhibited an unwillingness to have secrecy obtained by any other means than a *literal* cipher. I employ this term to distinguish from *syllabic* and *verbal* ciphers those in which the *letters* of the sentence to be concealed are separately expressed by characters, either simple or complex, in the cipher. I am not, of course, sufficiently acquainted with the diplomacy of the present day, to be able to state whether secretaries for foreign affairs, and their agents, are so precise in their ideas as to decline to be secret, unless they can be so *secundum artem;* but if they be, they are certainly more scrupulous than their predecessors of the 17th century were; and yet I much doubt if they can better baffle those whom violence or treachery might put in possession of their despatches.

My intention, in the present paper, is to expose the futility of *literal* ciphers; among the rest those of Mr. Blair, (REES's *Cyclopædia*, Article CIPHER,) and of Mr. Chenevix, (*Journal of Science*, &c., No. XIX.) I do not mean to assert, that no literal cipher can be contrived so as to be sufficiently secret; but that this cannot be effected without a key, so extensive and complicated that it might be applied with equal facility to syllables, as to letters. And if so, a syllabic cipher is unquestionably preferable; inasmuch as, being equally secret, it is written and read with about one-third part of the labour.

Of literal ciphers there are two kinds; one, in which the signification of the symbols is varied, the other, in which it is permanent. In the former, the number of symbols is generally small, and the cipherer depends for secrecy on the intricacy of the law according to which their signification is changed, or on the length of the period of the change. Under any circumstances this method is extremely troublesome; but, if the law of variation be not of the very simplest nature, the labour of both the reader and the writer of the cipher will be prodigious; and if the law be simple, it is plain that the period of the cipher can contain no more *loci*, (as Mr. C. calls them,) than the cipher contains characters; and this, I think, insufficient (with the usual number of characters, at least,) to defy the powers of a decipherer. I consider, in fact, this kind of literal cipher so obviously inferior to the other, that I should scarcely have noticed it, but for the purpose of explaining Mr. C.'s first specimens, (Nos. 1—12,) in which he has employed it.

The germ of this method is to be found in the dial-cipher of the Encyclopædia Britannica; and, in fact, the key to these specimens may be most commodiously exhibited in the form of a dial. Let there be constructed a circle of paste-board, moveable on a pivot in its centre, and attached to a larger circle concentric with it; let the interval between the two peripheries, and the rim of the inner pasteboard circle, be each divided into thirty equal compartments, and in the former let the alphabet, with Mr. C.'s four additional symbols, be arranged in the following order; *viz.*, a b c d e 4 f g h i j k 8 l m n o p q r s t ɸ u v w x y z φ; on the rim of the inner circle let the following letters be arranged, *viz.*, e e e a a i i n n o o s s t t r r u u b c d f g h k l m p y. Set the indicator, or capital letter prefixed to the cipher, to correspond with the first of the three e's, and in this position of the dial the signification of each letter in the outer circle will be the corresponding letter in the inner circle. This position of the dial will explain the first *division* of the cipher; for the following division the inner circle must be moved forward or backward, (as the position of Mr. C.'s arrow determines,) through one compartment; and so for the other division always moving the inner circle a compartment forward or backward at the end of each division. If a capital letter occur in the middle of the sentence, as in Nos. 2 and 3, set the dial for that indicator, and the key of the first division following it will be given.—Then proceed as before. In Nos. 1 and 6 the divisions correspond with words, in Nos. 9 and 10 with letters; in fact, they are perfectly arbitrary in their lengths.

By help of a key, constructed as I have described, it will be easy to read the concealed sentence, Nos. 6, *&c.*, which is this "Monuments of learning are durable;" and to correct the key, sentence, line 2, word 3, which should be "many" and not "five." Other errata, proceeding probably from the carelessness of the printer, are to be found in several of the ciphers.

It will be remarked, that in this cipher (and it is the case in all Mr. C.'s,) j must be represented by i, and v and w by u; but the letters q, x and z, are also omitted in the present key; and as none of them occurs in either of the sentences given, it is impossible to tell what contrivance Mr. C. uses to represent them; the letter marked k in the key will probably suffice for them all. But see Mr. C.'s character of a *complete* cipher, (p. 92). How can this be applied to any of those that he has proposed?

It would also appear from Mr. C.'s being able to cipher "Europe in 200 ways, Emancipation in 1280," (p. 93,) that he had *five* symbols for e in this key; there appear, however, to be only *three.* Has Mr. C. any contrivances to represent this letter, not exhibited in any of his 12 specimens? or are we to understand the passage in p. 93 as applying not to this variable key, but to the permanent one of No. 18?

I pass now to the second kind of literal ciphers, or those in which the symbols retain each a permanent signification. Secrecy is in such ciphers to be looked for, by having a considerable number of characters, especially to represent those letters which are of most frequent occurrence. It is evident that a sufficient supply of symbols for the most extensive key of this sort that could be required, would be furnished by the series of natural numbers, commencing suppose with unity, and continued *ad libitum;* a point, or comma, being interposed between every two for distinction. The letters of the Hebrew alphabet, accompanied by the several vowel points that may belong to them, would also afford an amply sufficient number of characters; but the difficulty of dictation by the person holding the key to his amanuensis, is no small objection to the use of these. A friend of mine has composed a cipher consisting of nine radical characters, those composing the well-known figure #,) which, with one, two, three, or more points at pleasure, above, below, or in the body of the character, compose a sufficient variety of symbols for any purpose; but the last-mentioned objection applies with even greater force to this system. Besides, the use of any of these sets of symbols, and especially of the two last, requires very complex characters to be frequently written; while, at the same time, it affords none of that *additional* secrecy, which the employment of professedly complex characters will often confer on a cipher. I shall return to the consideration of this additional secrecy. I now proceed to describe some of the principal ways in which cipherers have sought to procure a multiplicity of symbols, by combining together characters of a few different kinds. The varieties of the complex symbol will always be in number that power of the number of varieties in each part, whose exponent is the number of parts. In Lord Bacon's cipher there were five parts, and two varieties only in each part. This gave for the number of symbols 32, the fifth power of 2. In the line writing, (Plate 2, Cyclopædia, Art. Cipher,) there are three positions of the line, and three lines to a symbol, which gives 27 symbols. In the dot writing, (plate 3,) *four* dots composing a symbol, we have 81 symbols. Symbols composed of two letters are in number 676; if with the letters be mingled a point, as Mr. Blair has done, there will be formed 729 different symbols, the square of 27; and so in other cases. To commence with Lord Bacon's cipher. From the smallness of the number of possible symbols, 32, (of which, if my memory do not fail me, but 26, or 24, were in use,) it is plain that his lordship looked to elude the vigilance, rather than to defy the skill, of a decipherer. His professed object, if I recollect rightly, (but I have no access at present to his work,) was to express *omnia per omnia*, that is, to write any given passage in such a manner as to convey any given secret meaning; and this in such a manner as to excite no suspicion in the mind of any casual inspector of the writing that such secret meaning was involved in it. It is necessary in his Lordship's method that the secret meaning should be much shorter than the outward visible contents of the letter; not exceeding one-fifth of it. For the conveyance of very laconic messages this method is, I think, unrivalled; though an improvement would be required in the formation of the two alphabets, which, as exhibited by Lord B., are neither sufficiently distinct, nor sufficiently void of suspicion. Perhaps it might be better to compose the characters of five words, than of five letters, and to have the variation consisting in the correctness or incorrectness of their orthography. A mis-spelt word, or one improperly made to commence with a capital, or abbreviated, might supply the place of one of his Lordship's crooked letters; and if the outward epistle was that of a servant, or mechanic, or other ignorant person, this would create no suspicion; while the multitude of expletives used by such persons in their correspondence, would much facilitate the composition of the cipher. The employment of the names and style of such persons as I have alluded to, although in a somewhat different manner, has been, it would seem, a common trick in diplomacy; and, unless Privy-Councils and Secretaries of State were egregiously mistaken in their suspicions, the endeavours of an exiled monarch to recover possession of his throne, have been often shadowed under those of a turned-off servant to get back to his place, or of an ale-house-keeper to have his license restored.

I have spoken at greater length than I otherwise should on Lord Bacon's method, as it has been, I think, unfairly represented in Mr. C.'s paper. The difficulty of writing it is much exaggerated, and the real source of its secrecy is lost sight of.

I now proceed to Mr. Blair's different methods of secret writing. Of the dot or line writing with 27 characters, it is needless to say any thing, as it can have no pretensions to inscrutability. That with 81 has not yet received a fair trial, as Mr. B. accompanied his specimen with a key and interpretation, and the latter served to unravel whatever intricacy was attached to the former. I believe that, with a different and unknown key, it would be *possible* to decipher a long specimen of this sort of writing, but certainly not one of a few sentences only, or which concealed only the important words of the writing. I must however own, that I think still greater secrecy would be attained if, of the 81 characters, only about 40 were used to express the *letters* of the alphabet, and the remaining half applied to the *syllables* and *short words* of most frequent recurrence. The cipher of 729 characters has certainly no need of any additional contrivances to increase its secrecy, as with a new key, even arranged on a similar plan to the present one, which is by no means necessary, detection is entirely out of the question. But wherefore use above 700 literal characters, when 100 is sufficient to produce absolute secrecy; and even half that number, if syllabic characters be interspersed? The cipher is, beyond a doubt, sufficiently copious, without in the smallest degree diminishing its secrecy, to express by separate characters all the particles, pronouns, auxiliary verbs, and terminations, as well those that belong to the grammar, as those that make up the great body of the dictionary, and will have a number yet to spare for the proper names, and other words, that may be beforehand selected, as specially likely to occur in the correspondence. But to employ a cipher, possessing such means as this, in the manner Mr. B. has done, to denote *merely* the letters of the alphabet, appears to me a waste of time and trouble, to as little purpose as if he had turned the force of a steam-engine to draw a cork, or to crack a nut. What I have objected to the dot writing of 81 characters, will apply with apply with equal force to the figure-cipher which admits 100 characters, each composed of two figures. But, in his application of this method, Mr. B. has adopted a very happy contrivance, the description of which leads me back to a subject I promised to return to—the additional secrecy produced by the employment of complex characters, above what the same number of simple ones would create. This additional secrecy consists in this, that a person unacquainted with the key can never be sure before he commences the task of deciphering, how many of the simple characters he is to consider as coalescing into a single complex one. The difficulty thus occasioned is not eluded as easily as might at first be supposed, namely, by trying first one number, then another, till the right one be ascertained; for, of the elementary characters, some may compose perfect characters by themselves, while others are always found as ingredients in complex ones. This is the case in Mr. B.'s figure-cipher in the Cyclopædia. To accommodate it to the same key by which the other ciphers are read, it was necessary to reduce the number of complex characters from 100 to 81. This he accomplished by selecting one of the ten figures for a perfect character, while the other nine were always used in combination with each other. This 82nd character was used by Mr. B. as a null, making the intervals between the different words. By doubling it, and combining it with another complex character of the same nature, the different *stops* in a sentence might, with very little trouble, be expressed; but, if I recollect right, this has not been attempted by Mr. B. The existence of a single character of this sort may be detected by observing what elementary character is never found with an *odd* number of characters between its consecutive appearances; this rule however fails, if more than one such character be found in the system. Other contrivances increasing the confusion produced by complex characters to those ignorant of the key, but affording no additional trouble to him that possesses it, whether for writing or reading, have at different times occurred to me; and I have formed out of them a system, which I think possesses as much practical utility as any literal cipher hitherto published, the key being extremely simple, and yet of such a nature as even if it were intercepted, or partially discovered, not to make known the *entire* meaning to a person ignorant of a particular secret connected with it, which may be readily understood and remembered, but which could scarcely be guessed. This may be useful in case it should be necessary to commence a secret correspondence with a person abroad, to whom there is no opportunity of safely sending a copious syllabic key; for, if this could be done, I am not so partial to my own method as to think that this would not be preferable. Though I do not fear that any specimens I should give would be *correctly* and *fully* read, yet I shall abstain from making the trial at present, as I do not feel myself called upon to offer any *reward* for the discovery, and without one, few would probably choose to make the attempt.

It will now be expected that I should say something of Mr. Chenevix's ciphers 13—18. In these he has connected with the use of permanent characters a contrivance hitherto, I agree with him, never adopted. I own, however, I suspect the reason *why* it was never adopted, to be, not that it was never thought of, but that when thought of, it was always rejected. My objections to it are very great on many accounts, but as I do not choose to describe the novel contrivance, I must decline stating them to the public. The want of secrecy, which, when Mr. C. reads the remainder of this paper he must be convinced it possesses, is alone a sufficient objection to it. On this subject it is very plain that he has greatly deceived himself. His opinions that "the security is at least one hundred times as great with a double as with a single key," and that either of them exceeds in power "the limit $(m-1)^n$," (he should have said m^n-1), are both very erroneous, and show that he has very little acquaintance with the principles of the art of deciphering. Incorrectly as the specimens 17 and 18 are printed, especially the former, he will here see that *they have not escaped detection*, any more than those where a known sentence, ciphered by the same key, was given along with them. Perhaps Mr. C. may be surprised to learn that I deciphered No. 17 in considerably less time than No. 18. The frequency of small and common words it would therefore seem is of more service to a decipherer, and of course of more dis-service to the employers of the method, than the trifling difference of one or two keys being used in the different parts of the sentence. I am not very sanguine in my expectations of the reward offered for deciphering these specimens, and yet I think the following sentences comply with all the conditions. I first give the explanation of

Nos. 15 and 16. I have already given that of Nos. 6 and 12. I then give the *translations* No. 17, into the cipher used in No. 18, and *vice versâ;* thus showing that I can not only *read* but *write* in Mr. C.'s most secret ciphers. As to the composition of these, or similar keys, I am not aware that it is very difficult, when the method is so completely discovered as it has been by me. This Journal, at any rate, would not be the proper place for publishing the specimen of such a key.

Sentence ciphered in Nos. 15 and 16.

Should this sentence also escape detection, the security of the cipher will receive additional confirmation.

Substance of No. 17, in cipher of No. 18.

ljy a84naf cxkzoi rbq4i ϕgamtuy az imktz koop yammmfa 8gii vcca faayφ srdxfaϕ tzxlmpj qb4yxd mkmoiicvuw lddd 8rppϕqaussφ8d ii mdfzttrφdϕd faaffa mmm lsaxn kldbϕ wuyzkfmg.

Substance of No. 18, in cipher of No. 17.

iφjlbd8rrt pxcdϕφ ijmhismmpn tmcj zx isodzmp nmmmoix ndϕopp efgmtt stllxbprr xφxw4eee nfacrl oxahhaa wwoccoϕ dlxcmadvd cdoφ8pplf hisgdv llxcfcw mmrpp facimponn axy cfmldϕee jripmgxfs tt hhkmp ll plll8s glgamx wxlsfeaa4m gxsupϕϕ dfnmhgalm pw8fn tt cfcdqtt qklamk tzxt pgkx pccctq.

gh ddeaa zymijhe bdyφl ukifyyyy oyl lt8φoj lm.

The last sentence contains my address in the cipher of Nos. 13 and 16.

Art. IV. *Contributions towards the Chemical Knowledge of Mineral Substances. By the late* Martin Henry Klaproth.

[Continued from Page 280, of Vol. XI.]

Analysis of the Ferro-arsenical Sulphuret of Copper, or Fahlerz, from Freiberg.

The usual difficulties of ascertaining the relative quantity of arsenic where sulphur also is present, occurred in the examination of this ore, and being unable to obtain satisfactory results by any humid process, I proceeded as follows :—

A.

a. 200 grains of finely pulverized fahlerz were heated to redness in a small glass retort; the neck of the retort contained after this operation 17 grains of red *sulphuret of arsenic.*

b. The residuary ore had fused into a lead-gray mass ; it was pulverized, mixed with half its weight of charcoal powder and submitted to a second sublimation, the retort being kept at a bright red heat for one hour; it thus afforded a crystallized sublimate of pure *arsenic,* weighing 22.5 grains.

c. The residue was now taken out of the retort and exposed upon a red-hot plate so as to burn away the charcoal, during which the fumes of arsenic were also perceived to escape. After this operation the residuary ore weighed 146 grains, so that it had sustained a loss of 54 grains, which, however, cannot be regarded as correctly indicating the loss of arsenic and sulphur, since it is probable that during the combustion of the charcoal a portion of oxygen was absorbed so as to increase the weight of the residue.

d. The roasted ore was now digested in nitric acid diluted with its weight of water, and the blue solution passed through a filter, upon which there remained 7 grains of a mixture of charcoal and red oxide of iron, which being separated by muriatic acid, 3 grains of charcoal remained.

e. On adding the muriatic solution of iron to the preceding nitric solution, a portion of muriate of silver was thrown down, which being carefully collected and reduced, afforded 0.80 grains of *silver.*

f. The solution was now mixed with sulphuric acid and evaporated to dryness; the residue was perfectly soluble in water, so that it contained no lead. Excess of ammonia was therefore added, which separated *oxide of iron,* weighing after having been dried, mixed with oil and ignited, 45 grains; and being perfectly attractable by the magnet.

g. The dark-blue ammoniacal solution, supersaturated with sulphuric acid and precipitated by zinc, gave 82 grains of *copper*.

B.

The above processes afforded no indications of antimony, which, if present, probably escaped with the arsenic: 100 grains of the ore, therefore, were roasted without the addition of charcoal, and then digested in muriatic acid and dissolved by the aid of nitric acid, added drop by drop. The filtered solution, which was of a green colour, was concentrated by evaporation, supersaturated with potassa, boiled, diluted, and filtered. The filtered alcaline liquor after having been neutralized with sulphuric acid, and mixed with carbonate of potassa, was scarcely rendered perceptibly turbid.

C.

To endeavour to ascertain the relative proportion of sulphur to that of arsenic, 200 grains of the pulverized ore, mixed with half their weight of charcoal powder, were submitted to sublimation. A few drops of moisture and some fœtid gas was evolved, and the sublimate in the neck of the retort was opaque, reddish brown, and of a metallic lustre; but that in the dome of the retort consisted of pure *arsenic*. The sublimate weighed 35 grains: it was pulverized and digested in weak solution of potassa in a gentle heat; the liquid acquired a brown colour, and left undissolved 23 grains of metallic arsenic in the form of a heavy black powder; it afterwards deposited 1 grain more of arsenic, and the solution became colourless; the addition of acids separated the sulphur in the form of egg-yellow flocks. The colour of the sulphur, and the colourless appearance of its alcaline solution, indicated the existence of a portion of remaining arsenic.

Amidst these obstacles to accurate results*, I am induced to estimate the proportion of sulphur at 10 per cent., and to assume the following as the components of 100 parts of this ore:—

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* To the above Klaproth has added two other analyses of Fahlerz, but as they are nearly similar to that given in the text, and equally open to fallacy as far as concerns the determination of the weights of sulphur and arsenic, we have omitted them.

Copper	A *g*	41
Silver	*e*	.40
Arsenic		24.10
Iron	*f*	22.50
Sulphur		10
Loss		2
		100

Analysis of an Antimonial Sulphuret of Copper, (Graugültigerz,) from Kapnik.

a. 300 grains of this ore in picked and clean crystals were reduced to powder and digested in a mixture of three ounces of nitric acid, (sp. gr. 1.230,) and an ounce and a half of water. There was some evolution of nitrous gas, which was increased by heat, and when the action ceased the liquid was poured off, and the residue again digested in two ounces of nitric acid diluted with one of water; the insoluble portion was then collected and washed upon a filter.

b. To the pale blue nitric solution concentrated by evaporation muriatic acid was added, which produced a slight turbidness, and the mixture being heated, afforded only half a grain of a precipitate, which did not behave like pure muriate of silver.

c. The solution was next divided into three equal portions.

1. One-third was evaporated nearly to dryness, and the green saline mass moistened with sulphuric acid which presently formed a clear solution, indicative of the absence of lead; the solution was supersaturated with caustic ammonia, which left an insoluble portion of precipitate, which being collected and ignited weighed 3.5 grains. It was dissolved in muriatic acid, and the solution decomposed by prussiate of potassa. After the separation of the blue precipitate, carbonate of potassa occasioned a slight brown deposit which weighed 0.25 grains, and examined by the blow-pipe appeared to be *oxide of manganese*, so that the proportion of *iron* may be assumed at about 3.25 grains. The dark-blue ammoniacal liquor was now supersaturated with sulphuric acid, and the *copper* being separated by the immersion of a plate of clean iron weighed 37.5 grains.

2. Another third of the nitric solution was mixed with excess

of caustic potassa and boiled; when cold, the precipitated oxide was separated by filtration, and the filtered liquor being neutralized with muriatic acid was mixed whilst boiling hot with carbonate of potassa, a white precipitate was thus formed, which after ignition weighed 6.5 grains. This precipitate became yellow when heated, and again white when cold, which seemed like oxide of zinc; it readily formed a colourless solution with dilute sulphuric acid, and afforded crystals of sulphat of zinc. These 6.5 grains of oxide I consider as equivalent to 5 grains of *zinc.*

3. Since the long digestion in nitric acid rendered it probable that a portion of the sulphur of the ore had become acidified, the remaining third of the solution was devoted to the determination of its quantity; it was therefore mixed with muriate of baryta, and the precipitate when collected, washed, dried, and ignited, weighed 66 grains, which I consider as equal to 9.25 grains of sulphur.

d. The yellowish-white insoluble matter, remaining after the action of nitric acid, was digested in muriatic acid; the solution was of a straw-yellow colour, and the sulphur separated in yellowish flocculi; it was collected, washed with very dilute muriatic acid, and dried; its weight was then 57.25 grains; burned upon a hot plate it left 1 grain of residue, which being fused upon charcoal with borax, yielded a globule of *copper* weighing 0.75 grains.

e. The muriatic solution, concentrated by gentle evaporation, gave no trace of muriate of lead; it was divided into 3 equal parts.

1. One of these third parts was diluted with ten parts of water, which rendered it turbid, muriatic acid was therefore added so as just to restore transparency; hydro-sulphuret of ammonia was then dropped in, which gave a precipitate of a pure orange-colour.

2. The second third of the muriatic solution was largely diluted with water, and the white precipitate being collected and moderately heated weighed 30 grains. The remaining liquid saturated with carbonate of potassa afforded a greenish-gray

precipitate which consisted of oxide of antimony and copper, but which, when dry, scarcely weighed $\frac{1}{8}$ of a grain.

3. The remaining third of the muriatic solution was diluted with 6 parts of water, and rendered again transparent by an adequate addition of muriatic acid; a plate of zinc was then immersed, which effected the separation of 22 grains of metallic *antimony*.

The components, therefore, of 100 parts of the ore under examination are,

Copper	(*c* 1.)	37.50	37.75
	(*d* 1.)	0.25	
Antimony	(*e* 3.)		22
Zinc	(*c* 2.)		5
Iron	(*c* 1.)		3.25
Sulphur	(*c* 3.)	9.25	28
	(*d* 3.)	18.75	
Silver	(*b*)		together about 0.25
Oxide of manganese	(*c* 1.)		
Loss			3.75
			100

(Here follow five other analyses of varieties of the same ore, upon which the author observes, that copper, antimony, iron, and sulphur, are alone to be regarded as the essential components; silver, mercury, and zinc, being merely accidental additions.)

Analysis of a Cupreous Sulphuret of Lead and Antimony, from Clausthal in the Hartz.

a. It was ascertained by previous trials that the quartzose matrix of the purest parts of this ore bore the proportion of about 13 to 100.

226 grains, therefore, of the ore were heated to redness in a glass retort, but nothing sublimed, and the ore appeared merely to have undergone imperfect fusion.

b. It was therefore reduced to powder, and digested in 4 ounces of a mixture of equal parts of nitric acid and water.

The mixture was then further diluted with four ounces of water, and the digestion continued at a higher temperature till the residue acquired an uniform gray colour; the whole was then poured on a filter.

c. The nitric solution was concentrated by evaporation and mixed with muriatic acid, which produced no further change than that the original sky-blue colour became green; the addition of sulphuric acid, however, caused an abundant deposition of sulphate of lead, which being separated, the remaining solution was supersaturated with caustic ammonia, by which the brown oxide of iron was obtained, and after mixture with oil and ignition, it afforded 10 grains of magnetic *oxide of iron.*

d. The ammoniacal solution was mixed with sulphuric acid in excess, and a plate of zinc, then immersed, gave 23.5 grains of *copper.*

e. The insoluble residue of the ore was boiled in muriatic acid with the addition of a little nitric acid, and this operation was repeated with fresh acid so long as it acted; the remainder was placed upon a filter and washed, first with very dilute muriatic acid, and then with water. It weighed, when dry, 64 grains; its sulphur was burned off at a gentle heat, and left 28 grains of incombustible residue; so that the *sulphur* consumed amounted to 36 grains.

f. The remainder, boiled again in muriatic acid, was found to consist entirely of siliceous sand, and weighed 26 grains.

g. The several muriatic solutions were concentrated by slow evaporation and afforded abundant acicular crystals of muriate of lead; these were carefully collected, and the evaporation continued as long as they could be obtained; they were then washed with alcohol acidulated by muriatic acid, dissolved in hot water, and converted by the addition of sulphuric acid into sulphate of lead, the quantity of which, including that separated in process *c*, amounted to 120.5 grains, equivalent to about 85 grains of *lead.*

h. The remaining muriatic solution, now free from lead, and which by the test of hydro-sulphuret of ammonia appeared to contain nothing but antimony, was decomposed by a sufficient addition of water, and the precipitate collected, dried, and heated, weighed 51.5 grains, which by collateral experiments was found equivalent to 39.5 grains of *antimony*.

So that 100 grains of the cupreous sulphuret of lead and antimony consist of

Lead	(*g*)	42.50
Antimony	(*h*)	19.75
Copper	(*d*)	11.75
Iron	(*c*)	5
Sulphur	(*e*)	18
	Loss	3
		100

Analysis of the Sulphuret of Bismuth and Copper.

This is a massive ore of a steel-gray colour, soft, and giving a black streak.

A.

As a preliminary trial, 100 grains of the powdered ore were digested in a moderate heat with nitric acid; sulphur and fine siliceous sand remained: the solution was filtered, somewhat diluted, and tested by muriatic and sulphuric acids, neither of which caused any turbidness, so that neither silver nor lead were present. Water in larger quantity being added, a white oxide of bismuth separated, and a plate of iron, immersed in the residuary solution, threw down the copper.

B.

a. To determine the relative proportions of the ingredients of this ore, two hundred grains were boiled in muriatic acid, and nitric acid added, drop by drop, as long as it produced any action. The insoluble portion was carefully collected, washed and dried, and being duly heated the sulphur burned away to the amount of 17.5 grains. The residue was again digested in nitro-muriatic acid, and the portion which resisted its action was properly washed, dried, and heated so as to burn off the remaining sulphur, which amounted to 3 grains, leaving a siliceous residue weighing 37 grains.

b. The solution was evaporated till it acquired the appearance of a grass-green saline mass, which was redissolved and largely diluted, by which a milky mixture was obtained; this was set aside in a warm place, where it deposited a white precipitate, which was collected, washed and dried by heat: it weighed 94 grains.

c. To determine the proportion of metallic bismuth contained in this precipitated oxide, 100 grains of pure bismuth were dijested in muriatic acid, nitric acid being gradually added till the metal was dissolved; this solution was then evaporated in a sand heat to a saline mass, which was put into a large quantity of water, and the precipitate washed and dried as before; it weighed 122 grains. The remaining acid liquor was neutralized with carbonate of potassa, by which a further portion of oxide of bismuth, amounting, however, only to ¼ grain, was obtained. So that it appears from this experiment that the 94 grains of precipitate obtained in the analysis from 200 grains of the ore, are equivalent to 77 grains of *bismuth*.

d. The filtered liquor remaining after the separation of the oxide (*b*), was of a blue colour; it was saturated with potassa to separate the copper, which, though at first thrown down in the form of a blue precipitate, became, after having been some time gently heated, of a brown colour; it was collected, edulcorated, dried, and ignited, and was then found to weigh 70.75 grains, equivalent to 56.5 grains of *copper*.

Hence, 200 grains of this ore have yielded

Bismuth	77 grains
Copper	56.5
Sulphur	20.5
Silica	87
	191

But as the silica is to be considered as derived from the quartzose matrix of the ore, the components afforded by 100 parts of the pure ore will stand thus—

Bismuth	47.24
Copper	34.66
Sulphur	12.58
	94.48

[Klaproth attributes the loss, amounting to 5.52 per cent., to oxygen, and supposes that the bismuth is not contained in the ore entirely in the metallic state.]

Analysis of the Blue Iron Ore of Eckartsberg.

This ore occurs in kidney-shaped masses of an indigo-blue colour; a tint which it derives from exposure to air, for when recently taken from the strata in which it is found it is nearly white. It is soft and easily friable.

A.

a. 100 grains gradually heated red-hot in a small retort lost 20 grains, and acquired a brown colour. The loss consisted of pure water.

b. Exposed in a crucible to a more intense heat, the ore melted into a steel-gray slag, of a metallic lustre, and slightly magnetic.

B.

a. 100 grains of the ore were mixed with a solution of caustic soda, and evaporated: the dry mass being softened with water, there remained brown oxide of iron, which was collected, edulcorated, dried, triturated with a little oil, and ignited in a close crucible. It gave 47.50 grains of *black oxide of iron.*

b. The alcaline liquor was neutralized with nitric acid, and tested by ammonia, which afforded no precipitate: the ammonia being again supersaturated with nitric acid, acetate of lead was added, which caused a precipitate of phosphate of lead, weighing, after having been washed, dried, and moderately heated, 142 grains, equivalent to 32 grains of phosphoric acid.

100 parts, therefore, of this reniform blue iron ore, consists of

Black oxide of iron	(B *a*)	47.50
Phosphoric acid .	(B *b*)	32
Water	(A *a*)	20
		99.50

Analysis of the granular Chromic-Iron Ore, from Steiermark.

The specific gravity of this ore, freed from the talcose matrix in which it is embedded, is = 4.500.

a. 100 grains lost by moderate ignition 2 grains, and acquired a perfect metallic lustre: it was levigated, mixed with a lixivium containing 500 grains of caustic potassa, put into a polished iron crucible, evaporated to dryness, and moderately ignited for an hour; the mass at first frothed up, but afterwards entered into quiet fusion, and when cold was of a sap-green colour; it was readily soluble in warm water, depositing a reddish-brown precipitate, which was collected upon a filter, washed, dried, and digested in boiling muriatic acid. 23 grains remained insoluble, which were fused with potassa, and treated as before, by which the insoluble portion was reduced to four grains, and was ultimately rendered entirely soluble by another repetition of fusion and solution.

b. The muriatic solutions were mixed with caustic ammonia, by which a brown precipitate was formed, weighing, when duly washed, dried, and ignited, 35 grains. It was again dissolved in muriatic acid, when it left two grains of *silica*, so that the weight of the oxide of iron was 33 grains.

c. The green alcaline liquors were carefully neutralized by nitric acid, during which *alumina* separated, amounting, after having been dried at a red heat, to 6 grains.

d. The neutralized liquor, after the separation of the alumina, appeared of a fine orange colour. A cold nitric solution of mercury was added to it, till no further precipitation ensued, and till the supernatant liquid appeared colourless and transparent. The precipitate, which was of the colour of fine vermilion, weighed, when edulcorated and dried, 369 grains. The mercury was driven off by moderate ignition in a platinum crucible, and there remained pure *oxide of chrome* of a dark grass-green colour, weighing 55.5 grains. 100 parts, therefore, of the above ore, consist of

Oxide of chrome	(*d*)	55.50
Oxide of iron	(*b*)	33
Alumina	(*c*)	6
Silica	(*b*)	2
Loss by ignition	(*a*)	2
		98.50

ART. V. *A Letter respecting the Construction of a Balance, from* Capt. HENRY KATER*.

London, Aug. 8th, 1821.

DEAR SIR,

I OBSERVE in the last number of the *Quarterly Journal*, edited at the Royal Institution, the description of a Balance from a drawing by Mr. Children. I should not have troubled you merely to claim this as my own arrangement, but to notice an error in the description where the beam is said to be "of platinum;" it should have been "of bell-metal," which combines the essential desideratum *lightness* with the requisite degree of strength.

This beam had its origin in a wish to render the hydrostatic balance less expensive without diminishing its accuracy and sensibility, and it has in every respect answered my expectations.

Dr. Wollaston has contrived the following additional apparatus, which he finds particularly convenient. The annexed wood-cut represents the bottom of the case supporting the balance, A and B the scale pans. Four slips of wood, or brass, are connected by pins forming centres of motion at *a*, *b*, and *c*, the centre *c*, being fixed to the case. Four brass pins, *e*, *f*, *g*, *h*, of a sufficient length, project perpendicularly from the side pieces. These pins are to be brought into lateral contact with the scale pans by means of the ends *k*, *l*, when the beam is elevated above its support. The knife edge being then lowered upon the agate planes, and the pins withdrawn, any want of equilibrium becomes instantly perceptible.

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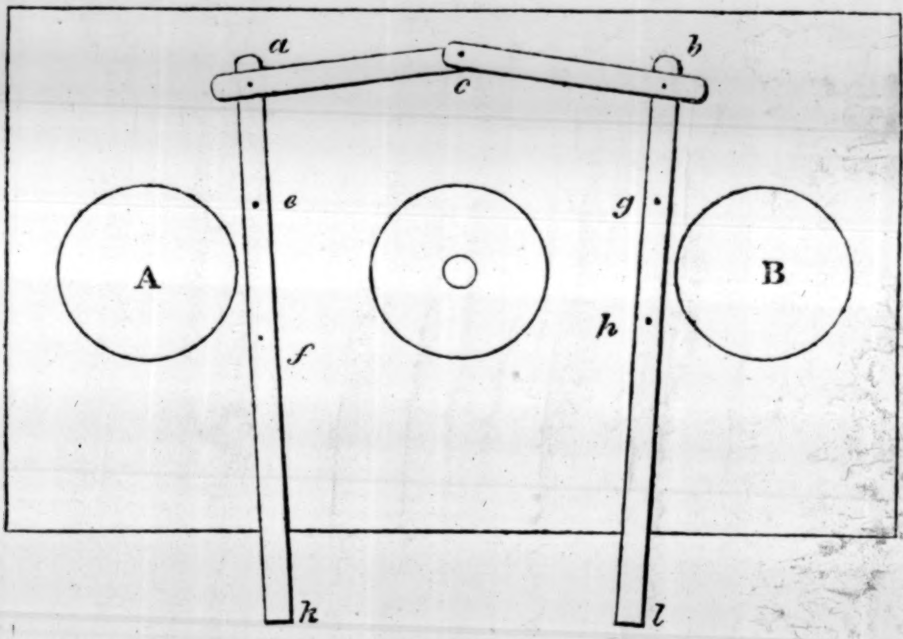
* We need scarcely observe that we were not aware to whom Mr. Robinson was indebted for the plan of the very useful balance of which we have given an account in our last Number. We merely saw the instrument in the possession of Mr. Children, who was kind enough to furnish the sketch from which our engraving was made.



I am, dear Sir,
Yours very faithfully,
HENRY KATER.

W. T. Brande, Esq.

ART. VI. *On Pitchstone.* *By* J. MAC CULLOCH, M.D. F.R.S., *&c.*

THE information to be procured respecting this rock is so scanty, from its limited occurrence and the imperfection of the observations hitherto recorded, that I cannot pretend to place its geological history in a very luminous point of view. There is reason to suspect that some of the remarks on its position have been warped by theoretic views of the same nature as those which have influenced the corresponding observations on the trap rocks; while it is also apparent, that the peculiar jaspers occurring in beds under trap, have been occasionally confounded with it, as I lately remarked in a communication on the subject of jasper. Under these doubts, I can only pretend to give its history as far as it can be deduced from the facts visible in Scotland. Hereafter, when it has been an object of more accurate research, its true history will, probably, be more completely elucidated.

In its geological connexions, as well as in some of its mineral characters, it approaches very nearly to many members of the trap family; although it presents, at the same time, some very remarkable differences. It is also important to remark, as it appears to strengthen the connexion between this rock and that family, that it is found in those districts or countries where trap exists.

It has been said to occur in the form of strata in many parts of the European continent; but no example of this disposition has yet been observed in Britain. Those masses which have been mistaken for strata in the island of Arran, are no more entitled to that name than the masses of trap which are so often found similarly placed; they are merely veins holding a parallel or conformable direction to the rocks by which they are included, as will be more fully pointed out immediately.

It will also be found that the stratified pitchstones of Europe, so often described, and so well known, do not appertain to this species of rock, and that the whole of the observations on these rocks are founded on a mineralogical error; that they have arisen from confounding the resinous looking jaspers, described under that head in a preceding article, with true pitchstone. These substances are, indeed, stratified; but they differ from pitchstone, not only in their several characters and chemical composition, but in every essential circumstance of their geological origin and position. This, at least, I may affirm of every specimen of pitchstone from these strata which has hitherto fallen under my inspection; and I have little doubt that a fuller and an unprejudiced re-examination of these imaginary strata, will confirm these views; by proving that they are truly jaspers, and that whenever real pitchstone occurs in masses parallel to stratified rocks, these are conformable veins, like those of Arran.

In every situation except one, where this rock occurs in Scotland, it forms veins, generally so decided in their characters, and intersecting in so marked a manner the accompanying strata at angles, as to permit no doubt respecting their real nature. In a few instances these veins are parallel to the beds in which they lie; but, in this respect, they differ in no way from the intruding masses of trap that so often occupy similar positions. These veins are rarely continued for a long space, more commonly terminating abruptly among the surrounding rocks. The ramification of these veins are so rare that I have hitherto observed it only in one instance; one of the pitchstone veins of Egg being divided round a mass of chert.

The only mass of pitchstone in Scotland of which the true position appears at first sight doubtful, is that which forms the Scuir of Egg, which is rather, however, a substance intermediate between that rock and basalt, and which is at the same time porphyritic. It stands insulated in the form of a narrow irregular wall on the surface of a mass of trap. So far it resembles a bed as little as it does a vein. But that it is not a bed deposited on the trap, nor a stratified rock, is evident from this; that the same mass of trap lies above the latest of the secondary strata containing coal, into which it also intrudes in the usual manner. If this pitchstone mass were, therefore, a stratified rock, it must be considered not only as later than the latest stratified rocks of which we have any knowledge in Scotland, but posterior also to the trap which succeeds these at a distant interval of time. Under any view, therefore, it is no part of the secondary strata. It is more probably the remains of a vein which has once existed in that trap on which it seems now to stand. The degradation of that rock may easily be imagined to have left the more durable pitchstone in the position it now occupies: and of this degradation, the surrounding rock, which is of a loose amygdaloidal character, gives ample evidence. The columnar structure is no objection to this view, as that structure exists frequently in trap veins. Neither is it an objection that this structure is parallel to the sides; since trap veins occasionally exhibit the same disposition of their columnar parts.

As there is so striking an analogy in many particulars between pitchstone and basalt, it is very conceivable that, if not found in regular strata, it may at least exist in the form of large overlying masses, like the trap rocks; and probably some of the foreign examples may be referred to this variety of position.

If the preceding remarks are not judged sufficient ground for excluding pitchstone from the regularly stratified rocks, the following argument will, perhaps, be considered valid. It is found in granite, in red sandstone, in the more recent sandstones, and in the latest trap rocks. It thus occupies a variety of discordant positions which no rock has yet been known to do except the intruding substances of the trap family. In granite, or trap, it is obvious that it could not be stratified; nor are we acquainted in nature with any rock which is found in the form of veins, and in that of regular and true strata also. Even where it occurs apparently inter-stratified among the sandstones, it must therefore be considered as a parallel vein.

Pitchstone presents some other remarkable analogies to trap. It is often porphyritic, or contains imbedded crystals, or irregular grains of feldspar, and occasionally of quartz also. This is an interesting circumstance in its history, as it is found only in the unstratified rocks, (with the exception of granitic gneiss,) or in those which intrude among the truly stratified substances in the form of veins. The porphyritic structure is, indeed, so common in the pitchstones of Scotland, that the instances of it far exceed those of the simple rock; particularly in the island of Arran. It is, perhaps, rare to find any mass which, in some place or other, does not contain imbedded crystals of feldspar, or, at least, rounded and irregular grains.

The amygdaloidal structure, which is so common in the rocks of the trap family, is, however, so rare in the pitchstones, that only one instance of it has yet occurred to me in the various specimens which I have examined. This specimen was from Baffin's Bay, and it contained zeolites, that is, mesotype or nadelstein; resembling, in every respect but its base, the well-known basaltic amygdaloids.

The entanglement of fragments of the including strata in trap veins, has always been esteemed a proof of the intrusion of these veins among the stratified rocks, and as further indicating a degree of violence, simultaneous with or preceding the intrusion of the vein. This very remarkable circumstance is sufficiently common in trap veins to have fallen under the observation of every geologist conversant in districts where these rocks abound; but in pitchstone veins it is so rare, that as yet I have only observed one example of it. This is in the well-known vein at the end of Brodick Wood in Arran. The including strata consist of the red sandstone, and the fragments of the same sandstone are insulated in the middle of the vein. If this occurrence is as yet solitary, it must be recollected that it does not happen, perhaps, in one trap vein of a thousand; so that it may be merely a question of chances, depending on the much greater rarity of veins of pitchstone. The fact, even if solitary, is valuable, as establishing, in another important particular, the resemblance between pitchstone and the trap rocks.

Admitting, or presuming, with most modern geologists, the igneous origin of the trap rocks, there is found in some pitchstones a remarkable circumstance, which, together with the analogies already pointed out to these, seem strongly to indicate an igneous origin for them also. Nothing indeed of this peculiar nature has occurred in any of the porphyritic traps that have come under my examination. In those pitch-stones which are of a porphyritic character, the crystals, or rather grains of feldspar, are rounded but irregular. When broken, it will be found that the feldspar retains its usual plated structure in the centre of the grain, but that it gradually becomes confused, or loses its character; while the outer surface is a coating of white enamel such as is produced by the fusion of feldspar. This feldspar is in some cases the glassy variety, in others the common; and, the change on the crystal is precisely that which may be induced by the regulated action of the blow-pipe. The smaller crystals, it is also worthy of notice, are entirely converted into enamel. These singular varieties occur in the hill of Glamich in Sky, as well as in Arran; and, it is interesting to remark further, the connexion which they possess with pearl-stone.

In tracing the progress of common pitch-stone to pearl-stone, it will sometimes be seen that a spheroidal concretionary, but indistinct, structure exists in some of these, which, by a gradually increasing distinctness of the concretions, causes them to pass into the latter substance. In others it may be observed, that the enamel grains just mentioned, are the centres of a spheroidal concretionary tendency; and that such varieties are in fact imperfect pearl-stones. In the pearl-stones of decided character, it will also be found that the grains or spherules contain a central enamel, and these varieties have been called pearl-stone porphyry. It is, moreover, interesting to observe, that in other cases the enamel and the investing pitch-stone spherule seem to have entered into combination; the result being a pearl-stone of a distinct and peculiar character, but in which the enamel grains may still in some part or other be discovered.

I may, lastly, remark, while on the subject of the porphyritic structure, that in some rare instances, of which Arran affords an example, the feldspar crystals consist of successive concentric prisms, a layer of pitch-stone being interposed between each, so as to produce a compound crystal.

There is no stronger proof of resemblance between pitch-stone and the trap rocks, than the transition from that substance to basalt, a change which is far from being uncommon. The rock which forms the Scuir of Egg already mentioned, holds a place pretty nearly intermediate between the two. Nor are similar transitions rare in basaltic veins in many other cases; particularly where one such vein traverses another of a larger size. In many places, and remarkably at Loch Scavig in Sky, the basaltic ramifying veins that traverse the hypersthene rock, become gradually finer in texture as they divide, until the minute filaments are converted into perfect pitch-stone.

In the same island, a vein of basalt of a considerable size is found in the Cuchullin hills, the outer laminæ of which are pitch-stone, graduating into the common matter of the vein. But Lamlash presents the most striking instance of this nature: here a vein of basalt passes through a vein of greenstone; and the outer parts or walls, to the depth of about half an inch, are formed of a glossy black pitch-stone. In some parts of its course this outer lamina is very decided and distinct, but in others it graduates into the basalt of the vein by an intermediate substance similar to that which forms the Scuir of Egg. A confirmation of this peculiarity, though of a less marked nature, is found in one of the well-known veins of Egg, where the outer part of the vein is a remarkably perfect and brittle pitch-stone, while the inner is formed of the intermediate substance just described.

From all these circumstances it may be concluded that pitch-stone possesses a strong affinity to the trap rocks, both in its geological relations and in its origin, being among the latest of the intruding rocks.

In one respect it presents an important character of difference. While in all cases veins of trap are very persistent, and can in many places be traced to a fundamental mass, those of pitch-stone are remarkably limited in extent, and have never yet been found connected with any principal body of the rock.

Veins of pitch-stone vary considerably in breadth, ranging from many yards to a few inches. In the case above alluded to, where basalt veins become pitch-stone during the progress of ramification, they descend almost to the dimensions of a thread.

In composition they are not always and invariably the same throughout. Independently of the rare cases in which veins composed of basalt in the middle are formed of pitch-stone at the sides, it sometimes happens that pitch-stone of many different qualities will be found in the same vein. In these cases the most perfect or vitreous-like parts are placed as in the former instances, at the outer sides of the vein; and, in the same cases, they present differences of colour as well as of texture and aspect.

Occasionally, these veins like those of trap present a laminar disposition, the lamina being parallel to the vein; and, in such instances, it sometimes happens that there is a change of substance where each lamina terminates; the pitch-stone passing into chalcedony and into chert. In a few instances they present a prismatic tendency at right angles to the course of the vein; and if the Scuir of Egg be also a vein, they must further be conceived capable of dividing into regular columns parallel to its sides.

In respect to the internal disposition of pitch-stone as a rock, it is various, presenting many remarkable peculiarities of structure, both on the great and small scale. In Arran, it is found imperfectly prismatic on a very large scale; the prisms being at right angles to the plane of the vein. In Egg, the prisms are smaller, but capable of being easily detached, in many cases, and often of very perfect forms; being at the same time placed in various intricate directions with regard to the plane of the vein, and in such a manner, that the ends of the several prisms are extenuated, by being compressed between the middle parts of those adjoining. The laminar disposition of the veins noticed in a preceding paragraph, sometimes also occurs on a scale so large as to divide the vein into two or three separate portions, which are most frequently marked by some corresponding change of character in the different laminæ.

The smaller varieties of structure are numerous, and often very remarkable. Among these, a laminar concretionary structure is not unfrequent; and the lamellæ which vary much in size, are either straight or curved. In some cases they are as thin as paper; in others they are thick, and further separable by joints at right angles to the planes of the lamellæ. Thus they become divisible so as sometimes to present a minute prismatic disposition.

Occasionally these prisms present also some curved surfaces together with the straight, so as to form columns on a very small scale, which are further jointed, in some very rare instances, by surfaces alternately concave and convex; and, when such joints are very near, the parts separate into irregular spheroidal forms. In some rare cases of the jointed columnar structure, a central atom of feldspar or of enamel, is found in each joint. This structure presents a considerable resemblance to that of pearl-stone, here considered as a variety of pitch-stone, and the specimens of this nature actually pass into that substance.

Analogous to this is a spheroidal concretionary structure more or less perfect, and varying in size, but commonly minute. By a transition from this, or by the greater perfection of the spheroidal structure, pitch-stone becomes at length an aggregate of irregular grains; and it thus passes, as in the former case, into pearl-stone, which, from this and the preceding fact, can only be considered as a variety of pitch-stone, and is accordingly ranked here as a variety. The spherules or grains of pearl-stone frequently also contain a central atom of feldspar or enamel as already noticed, like the variety mentioned in the preceding paragraph; a circumstance which confirms this view of its nature, and which is perhaps very intimately connected with the peculiarity of its structure.

The last obvious variety of structure to be noticed in pitchstone, is the porphyritic, already mentioned in pointing out its analogy to the trap rocks. This varies much in the magnitude, perfection, or number of the included crystals, producing corresponding differences of aspect. It was already observed, that the larger crystals are rounded, and converted into a white or grey enamel on the exterior, while in lieu of the smaller there are only to be found spheroidal grains formed entirely of the same enamel.

Independently of these obvious varieties of structure, pitchstone sometimes gives indications of an internal arrangement, which, as in the case of the trap rocks, is only detected on wethering. Many of the varieties become of a pure white on the surface after exposure to air, scaling off in successive crusts that resemble white enamel, as they still retain the vitreous aspect and fracture. At the same time the exposed surfaces are covered with undulating lines, resembling certain varieties of marbled paper, and evidently resulting from some corresponding difference of laminar structure; the lamina most sensible to atmospheric action being thus detected, although invisible in the fresh rock. In a further progress to decomposition, pitch-stone is at length converted into a fine clay, forming in water a very tenacious paste; and it appears to be very readily acted on by the water, as may be witnessed in Arran. It does not always weather to a white enamel, becoming occasionally brown, and more rarely turning into a black powder. Occasionally it exhibits the kind of bloom seen on a plum, or on certain kinds of bottle-glass.

It must here also be remarked, that notwithstanding its vitreous aspect, pitch-stone often contains a great deal of loose water, which is easily separated by drying, as in many other rocks. In these cases it is tender, but becomes brittle after the water has evaporated.

The lustre of pitch-stone varies exceedingly, according to its several states or varieties; passing from the most perfectly vitreous to one scarcely more glossy than that of the finest basalt.

The colours are various, greyish white, pale ochre-yellow, brown, olive brown, olive green, dark reddish brown, dark bottle-blue, and black. In Scotland the darker colours, and the olive greens in particular, are predominant.

The ordinary transitions of pitch-stone are into chalcedony, chert, and semi-opal: it appears also to pass into a substance not easy distinguishable from the conchoidal shining jasper found among the clay strata that are entangled in trap or volcanic rocks, with which, as I formerly remarked, it appears to have been occasionally confounded. The transitions into basalt have been already noticed.

Such is a general sketch of its geological and mineral characters, as far as my limited knowledge extends. The more minute particulars will be found in the synopsis of varieties.

It is scarcely necessary to define it, further than to say, that in a general way, it is distinguished from all other rocks by its vitreous or by its resinous aspect; and that it differs from obsidian, with which it possesses, in fact, no affinity or transition, by its inferior hardness. For a fuller definition I may refer to the various systems of mineralogy which are in the hands of every one.

SYNOPSIS OF PITCHSTONE.

FIRST DIVISION. SIMPLE.

A. Amorphous, massive.

a. With a simple conchoidal fracture.

b. With a flat conchoidal fracture.

c. With a splintery conchoidal fracture.

d. With a splintery granular fracture, and of various aspects, according to the size of the parts.

e. With a mixed fracture, the conchoidal being also granular.

f. With a mixed fracture, the large conchoidal presenting a minute additionally conchoidal surface.

g. The conchoidal fracture covered with minute scales, so as to give, on a superficial view, a porphyritic aspect.

B. Concretionary.

a. Flat, lamellar; the lamellæ thick or thin; it even becomes schistose or papery, or fine scaly.

b. Curved lamellar.

c. Prismatic; the prisms simple or jointed; with plain or with curved surfaces.

d. Columnar; similarly, either simple or jointed: the concretion is also sometimes partly columnar, and partly prismatic; or has plane and curved sides.

e. Spheroidal; large. This is generally combined with the columnar. The small spheroidal concretionary, is arranged with the pearl-stones.

f. Very imperfectly spheroidal, concretionary; also passes into the porphyritic.

§. Sometimes different structures are intermixed in the same rock, as the columnar and prismatic with the lamellar. The lamellar pitch-stone sometimes contains lamellæ of chert, or of cherty chalcedony, into which it also passes.

SECOND DIVISION. PORPHYRITIC.

A. Pitch-stone porphyry of mineralogists.

a. With distinct crystals of glassy feldspar.

§. These are sometimes so transparent as to be nearly invisible on a fresh fracture.

b. With distinct crystals of common feldspar.

c. With rounded, or shapeless particles of the same.

d. With imbedded spherules, consisting of a grain of feldspar surrounded by a grey enamel, or of the grey enamel alone.

§. This enamel is sometimes blended with the surrounding rock. When the spherules are numerous, this porphyry passes into pearl-stone.

e. With imbedded crystals or grains of quartz, or of quartz and feldspar both.

f. With grains of quartz surrounded by chert, and blending with the surrounding rock; or with grains of chert alone. This variety is analogous to *d.*

g. Porphyritic; but the grains, which are chiefly of quartz and chert, are further condensed in lamellæ, which alternate with a slightly porphyritic pitchstone.

THIRD DIVISION. CONCRETIONARY-SPHEROIDAL: PEARL-STONE.

A. Consisting of simple pitch-stone; the grains irregular, and compressing each other in the manner of coccolite.

B. Some, or all of the grains, containing a central substance of another kind; pearl-stone porphyry.

a. With grains of feldspar, or of feldspar and enamel, or of enamel only.
b. With quartz, or quartz and chert.
c. With both quartz and feldspar.
d. With a central atom of clay.

FOURTH DIVISION. AMYGDALOIDAL. *Containing imbedded nodules of another Mineral.*

A. Pitch-stone containing imbedded zeolites.

§. As yet this variety has occurred only in Baffin's Bay.

§. The colours of pitchstone are various. They are principally dull white, pale ochre, pink, pale green, greenish grey, ochre yellow, ochre red, yellow brown, fawn colour, red brown, greenish brown, olives of various hues down to blackish green, dark blue, and black. These colours are sometimes also intermixed; and thus, pale and dark green, or pale and dark grey, are interlaminated, producing a striped surface. The colours of pearl-stone are much more limited, possibly because the substance is more rare. Those of the porphyritic varieties are numerous, as this rock is more frequently porphyritic than simple.

§. As the porphyritic varieties may possess any of the modifications of the simple rock as a base, many additional aspects result from this cause.

§. Besides the passage into chert and cherty chalcedony already noticed, pitchstone passes into basalt, and, as is supposed, into opal and semi-opal. In the passage to chert, it is sometimes found to contain minute grains or particles of chalcedony, discernible only by the lens. This variety often possesses a remarkable spheroidal concretionary structure, which was mentioned in a preceding part of this communication.

ART. VII. *A Translation of* REY'S *Essays on the Calcination of Metals*, &c.

[Communicated by JOHN GEORGE CHILDREN, Esq. F. R. S., *&c.*]

Continued from VOL. XI. p. 271.

ESSAY XI.

Air is rendered heavier by the separation of its lighter parts.

IN discussing the third method by which air becomes heavier, namely, by the separation of its lighter parts, I begin with this incontrovertible truth, that if from any thing its lighter parts be taken away, the remainder will be heavier; I do not say that it will be heavier than the whole original quantity, but than a portion of it equal in bulk to the remainder. Separate the silver which that knave of a goldsmith mixed with the gold in King Hiero's crown, the remaining gold will be heavier than an equal portion of the entire crown; what in this case you do by art, nature effects by her industry, having no other tool but heat, which serves her worthily in the work. Observe the preparers of salt, who receive the sea water into their pans, through canals; they know that the sun's heat subtilizing the water, will sublime it into air, leaving them the salt, the heavier part, at the bottom. The alchymist, true ape of nature, desirous to imitate her, places his infusion of rhubarb over a chafing dish, that the liquor exhaling the extract may remain behind. But if he want that part, which being subtilized, flies off, he stops it by the way, (sly fellow as he is) *(Cauteleux qu'il est,)* by means of the heat which he applies to his alembic. By this scheme he obtains the brandy, which is lighter than the wine, from which it rises, and the wine lighter than the lees remaining after the whole distillation. In this manner heat acts on all sorts of liquors, rarefying some parts, thickening others, and always separating them by the difference it occasions in their respective weights, *(par peser plus ou moins.)* It produces the same effects on air; to observe which, turn your face, I beg, towards that plain over which the sun has all the day darted his rays. You think, I wager, that the air which is in immediate contact with it, is lighter than it was in the morning? just the reverse; it is much thicker, and consequently heavier; for how can the heat have subtilized the air, without having caused it to ascend, and how have caused it to ascend without the descent of a heavier? Nothing rises by its own power, it is the falling down of another body which pushes it up. Doubtless it has separated the most subtle, and raised it on high, leaving the heavier below, as happens in the distillation of liquors. But if this argument do not convince you of the thickening and increased weight of this heated air, at least believe the evidence of your own senses. I undertake to make you feel that it is so by the touch of your hand, and see it with your eyes. Now, whilst it is mid-day, do you not feel the air warmer to the touch, than it was a quarter of an hour after sunrise? It is not however that the sun has imparted a higher degree of heat to it, since that never varies, and it disperses it at a constant rate within the sphere of its activity; for in like manner as its rays transpierced the air without resistance, and communicated all its light to it in a moment, so did it communicate its heat, which has not itself increased, though its action is greater, from the greater density of the matter it acts on; for the more subtle parts being gradually raised up, the others remain below in greater number, and more united in the self-same place; and from this greater union comes this greater action. This may be made clearer by considering the nature of elementary fire, which although hot in the utmost degree, nevertheless does not burn, in consequence of its extreme rarity, but red-hot iron burns violently, not that it is hotter (for how can its heat exceed the utmost degree) but because it is denser, containing more parts in an equal space. So much for the touch, let us proceed to the other sense. When the sun began this morning to send its rays on the horizon, the air, by its subtlety, was wholly invisible; but now do you not see how it trembles over the furrows? It is because it is thicker, and has acquired more body that it is in some measure visible to you. Thus, I think, I have sufficiently acquitted myself of my promise; but it is fit that I should go a step further, and add, that if the simple heat of the sun thus thicken our lower air, and make it heavier, driving upwards its more subtle parts, and keeping the denser and more solid *(drues)* below, what will not the vehement and long-continued heat from the mouth of a red-hot furnace effect? A quantity of brandy placed over it in a vessel will disappear, common water, and all sorts of liquors, will exhale in a few hours. The air nevertheless will remain there, (there being no other body to fill its place) but it will be an air of the utmost possible density and weight; an air, as I may say, no longer air, but that has lost its nature, having changed its subtle fluidity for a viscid grossness, *(grossiereté visqueuse.)* For the violence of the fire, subtilizing all the air that comes near it, will drive an immense quantity of it to a distance, leaving around itself, of this immense quantity, only a kind of dregs, which from its glutinous weight cannot fly off.

ESSAY XII.

Fire, by its heat, can thicken homogeneous Bodies.

I know not what fatal calamity has seized the sciences, that when an error has arisen in them, and they are, as it were, inured to it by time, their professors will not endure its abatement. The doctrine of the preceding essay has already been taken in dudgeon, *(formalisé)* and it has been objected to me, that since fire thickens heterogeneous bodies, by separating their more subtle parts, as being of a different nature, it cannot do the same with homogeneous bodies, inasmuch as it acts uniformly on all their parts, and has no other action than that of distending and dilating them equally; so that, on this account, air cannot be thickened and made heavy by the force of heat. I recognise this doctrine, (thus opposed to my belief,) as derived from the school of philosophers, whom I honour for their great insight into nature; but I frankly avow, that I have never sworn by the words of any of them. If truth be with them, I receive it;—if not, I seek it elsewhere. Let us see if they have met with it in this matter. Homogeneous bodies, they say, are those whose parts are all of the same nature, or which have the same name and definition as the whole. I certainly agree that fire acting on such bodies, does, of its own accord and nature, *(de soy et par sa nature,)* dilate them: but reason teaches me, and experience confirms it, that, by accident, as we say, and in consequence of the subtilization and separation of some parts, the others remain thick and heavy. If that be denied me, and the aforesaid doctrine must be rigorously investigated, as if fire, neither by itself nor by accident, could thicken homogeneous bodies, I challenge it as false, and could bring forward a cloud of instances to the encounter; but the courteous reader, for whom I labour, will be contented with a few.

Vitriol is a homogeneous body, for its parts have the same name and definition as the whole. Now, if this be put in a retort, fire so acts upon it, that it shews us separately its phlegm, its oil, and its colcothar, parts which differ in thickness and weight. Turpentine is a homogeneous body, the smallest part being no less turpentine than the whole mass; if this be put in an alembic, fire, by its action, dilates some of its parts, and thickens others, separating its water, spirit, oil, and colophony, whose difference in respect of weight and subtlety is notorious. I have already spoken of wine, (also a homogeneous body,) which fire, displaying its powers on it in distillation, extends and dilates till it extracts its brandy and weak water *(eau petite,)* as it is called; and the residuum is thicker in proportion to the quantity of this water or phlegm that has been drawn off. But why do I take the trouble to bring forward these examples, since it is evident, that from all such bodies, salt, sulphur, and mercury, parts which sensibly differ in tenuity and weight are extracted by the help of fire; it is not true, therefore, that fire dilates all their parts equally. I well foresee that some will endeavour to evade this, by saying that the instances I propose are of compound bodies, and that it would be otherwise with those that are simple. Yet, forsooth, have I proved the falsity of this maxim, taken, as they lay it down, in its general sense, and extended to all homogeneous bodies. But let us see if it be more consistent with truth when applied to simple bodies.

ESSAY XIII.

Fire can thicken Water.

Water, unquestionably, is a simple body, and it is equally certain, that fire acting on it, dilates some of its parts, and thickens others; albeit, as I have said already, the first action is proper and natural to it, and the second accidental. Pour a barrel of water into an alembic, heat it according to the rules of art, and draw off first a gallon *(pot*)* of it. It is certain that the water of this gallon will be more subtile than that which you put into the alembic. If any one stimulated by the desire of contradiction deny it, let him be refuted by the chemists; who not being able to make their extracts conveniently with common water, are accustomed to use distilled water or dew, (which is nothing else than water passed through the great alembic of nature,) for such water being more subtle, better penetrates the substance of the simples, and more readily draws out their virtues and tinctures. Moreover, its greater diuretic effects, and its less weight, (the inseparable companion of a less degree of thickness), give ample proof of the truth of my assertion. But if the water of this gallon be more subtle than the water put into the alembic, that which remains must be thicker, inasmuch as a thickening is a necessary consequence of the separation of the subtle part. This will be more evident if you continue the distillation; for, drawing off gallon after gallon, till no more remains, the last will be sensibly heavier and thicker than the first; and this sensible difference will ensue by small, but imperceptible, degrees, from the first to the second, from the second to the third, and so on consecutively to the last. Nor will this difference be only from gallon to gallon, but from glass to glass, indeed, from drop to drop; for since the two extreme drops must manifestly differ in thickness and weight, this difference must continually be increasing from the beginning to the end, by the increase of the number of drops arising from the distillation. From this it appears, that, as in heterogeneous bodies, fire separates the parts that are of different natures, so in homogeneous bodies, it disjoins the parts which differ in tenuity, and then weight assumes the office of giving them rank, and of assigning to each its place, especially in fluids, the heaviest portions of which, always gain the bottom, making their way through those that are less so, and necessarily falling down in them. So that if all the water that could distil from the above-mentioned pipe, fell in order into a tube of sufficient length, and of the size of a quill, it is credible, that the second drop would sink in the first, and the third in these two, and so consecutively to the last, which being the heaviest, would pass through all that preceded it, occupying the lowest place; so that the drop which sought the first, would, at the end, find itself in the highest place. Now, although this continual traversing occasions some degree of mixture to the parts, yet would it not be such, but that the distinction in weight of the high and low portions would always be very discernible. But as we cannot see this sinking of the drops by the eye, if any one call it in question, let him apply and dexterously join the mouth of one phial full of water to the mouth of a similar phial full of claret wine, and he will behold a similar thing; for the water being the heavier, will descend into the lower phial through the wine, manifestly forcing it to mount into the upper. Does not the wine itself arrange its more subtle parts at the upper part of the barrel, and its grosser at the bottom, by means of the greater weight of one than of the other?

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* A measure nearly equal to a gallon.

The common people think also, and not without reason, that the first glass we pour out of the gallon is more subtle and vaporous than the following. This difference observed in so small a vessel might lead some to imagine, that if we were to make a tube only an inch wide, and several toises in length, fill it with wine and leave it for some time at rest, the upper portion, if not absolute brandy, would come very near it in tenuity and efficacy. A pretty invention, certainly, to draw off brandy without fire, if the fact be so, (*si la chose allait ainsi*;) and the difficulty of making the instrument did not prevent the use of it. All these observations serve me as a bridge to pass to this general assertion, that in all fluids, as well compound as simple or elementary, the upper parts always differ in subtlety and weight from the lower; and that this difference is distinguished by as many degrees as their matter is divisible by their height into distinct parts: so that if we conceive a line drawn from the lowest part of one of the fluid elements (as air) to its highest surface, just so many degrees of weight and subtlety will there be in that element, as the line can be divided into different parts, (I mean materially to avoid sophistry,) and the upper part of all will always be thinner and lighter than the second, the second than the third, and so on to the end. For to attribute to all the parts of each element the same body, (*corpulence*) is to belie our sense, which compels us to consider air (for instance) more subtle at the summit of a mountain, than in the plain at its foot. And, in like manner, when the heat of the sun, or of our fire, subtilizes it here below, it mounts on high, unquestionably, till it meets with its like, according to the degree of subtlety it has acquired. Besides, if this equality prevailed through the whole elements, there would be no reason why one portion of it should be below, and one above, when the air is calm. For to commit that to chance and hazard, would be to shock the incomparable wisdom of the Author of nature, who has made nothing in it without number, weight, and measure; and has established such order throughout, that nothing happens fortuitously and without cause. I conclude, therefore, that this arrangement is derived from weight, and no otherwise. But to conclude this essay, I say that every one may now see that fire acting on the simple body, water, does not equally distend all its parts, but by dilating some it separates them, whence follows the thickening of others. Thus the maxim in dispute is not true. But it will be said this must be shewn of air, on which the pivot of the controversy turns. That is their last refuge, and I proceed to deprive them of that.

ESSAY XIV.

Fire can thicken Air.

THE reasons discussed in the 11th Essay, are sufficient to satisfy an unprejudiced mind that fire heating air subtilizes and separates some of its parts, and that this separation is necessarily followed by the thickening and increase of weight of the others. But since this truth is obstinately contested, in order to exhibit it more clearly, I demand that a laboratory be prepared for me in the region of elementary fire, adjoining that of air, and in it I will shew them ocularly what they are unwilling to believe. For as the vessels which we here call empty are nevertheless full of air, so will they be there full of fire. And since when we pour water into an alembic, the previously-enclosed air is expelled, so will fire give place to the air which in that region shall be poured into the alembic; and when put upon the furnace will distil over drop by drop into the receiver, and the first measure of it that is collected will be more subtle than the second, the second than the third, and so on to the end. What is more, the difference in subtlety and weight between the first and last measure will be as perceptible as in those of distilled water. Now if any one laugh at my demand, let him know that the great Archimedes required, in a like case, that a spot should be given him in the region of air where to place his engine, and he then promised to lift up the whole earth. (*Δος πȣ στω, και την γην κινησω.*) Not that he supposed that what he demanded could be done, (for in the estimation of the wisest men he was neither madman nor fool,) but he made it, confident in the certainty of his demonstrations, and for clearer evidence of the truth of his assertions. My demand too has no other object. Whoever would see a thing approaching to this, without having recourse to an impossibility, let him place an alembic of extraordinary dimensions on a furnace, and having fixed a bladder, emptied of its air, to the highest part of the head by a small pipe, begin to heat it; the air of the alembic will then begin to dilate, and being no longer retainable in its original space, will rush out and fill the bladder. Let another similar bladder be filled and fitted in like manner, and so on to the end. I say that the last bladder will be heavier than the first; whoever doubts it, let him make the experiment, proceeding exactly as directed. By this train of thought my mind elevates itself to greater things, which, however, I shall not mention now, as they are irrelevant to this subject, and difficult not only to practise but even to understand. I come to another demonstration, by which the truth that I defend will be more than evident (*plus que visible.*) Plant a cannon directly on its breech, its muzzle pointing upwards, and throw into it a red-hot ball of the size of its bore. It is certain that the air contained in the cavity of the gun is so thin in substance and small in quantity, that the ball, in passing, will impress on it its full degree of heat; notwithstanding this, if you put your hand into the muzzle, you will easily keep it there at first, but in a short time you will be unable to do so; not that the air has increased in heat, for it will rather have decreased, as well as the ball which gradually cools; but, because being thickened by the separation of the more subtle parts of a large quantity of air which will rush out quickly, (*d'un abondance d'air, qui s'y portera grand erre*) it will act more powerfully, as I have said elsewhere. In the second place, the air which will be seen trembling above the muzzle of the gun, (which does not take place at first,) compels us to confess, that it is thickened; for, it cannot be said that these are vapours or exhalations rising from the gun, for every thing about it is too dry and solid to suffer the escape of any of its own substance. Thirdly, if the air were not thickened above the muzzle it would not render the objects beyond it, seen through it, confused. Nor can my opponents excuse their disbelief on account of the tremulous motion (*brandillement*) of the air, since I distinctly see that lady's beauties through the air, which she agitates with her fan, and also all sorts of objects through an atmosphere violently disturbed by the north wind, when it blows and whistles boisterously. Lastly, if a flock of wool much extended (*esparpillé*) be placed over the muzzle of the gun, it will not descend; and, if it be pushed some distance into the mouth, it will suddenly rise up again, which would assuredly not happen if the air were not thicker than on (*à l'escart*) one side of the cannon where the flock of wool descends readily. These reasons though not gross, are nevertheless so palpable that they must convince every one that heat has thickened the air just above the muzzle of the gun. But if it has thickened it before the mouth, what, I pray you, must it have done at the bottom of the bore next the ball? Unquestionably when that comes out after it is cold, you will see it more inclining to a whitish hue than before it was heated red, as if the thickened and adhesive air gave it that colour, which in time tarnishes and wears off, especially in a damp place, since the surrounding air, moistening that which adheres to the ball, reduces it to its original state. By way of dessert I will serve up one remark to the reader, which perhaps will be to his taste. They who worthily practise medicine are sometimes called in to visit an asthmatic patient, who panting in bed in a hot chamber, fetches his breath with great difficulty, which the physicians observing, they have the window thrown open, conduct the patient to it, and make him inhale the outer air, to his great relief. If you ask these gentlemen whence the sick man receives so immediate comfort, they will tell you, it is because the air of the chamber being too hot cannot furnish that necessary refreshment to the heart, which the external air by its coolness affords. But gentlemen, my honoured colleagues, having undeceived myself on this point by the preceding meditations, suffer me, I pray you, to undeceive you also. It is not the heat of the air of the chamber that occasions the panting, as not being capable of sufficiently refreshing the heart, but rather it is its thickness, which retards its course across the *obstruction* (*literal*) of the lungs, so that it cannot furnish the heart regularly (*à temps*) with sufficient matter for the generation of the vital spirits, which fresh air being more subtle, can better effect. Now, that you may not fancy I advance this without reason, observe the feverish patient who lies in the same chamber, whom the confined air sufficiently refreshes, although he has greater need of it; and, if fever were to come on to the asthmatic patient (which you wish for his advantage,) and dissipate the matter that closes the vessels of the lungs, would not the same air then sufficiently refresh the sick man, now that the need of it is increased? Does not the same thing happen if this clearing of the lungs be affected by the diasulphur, which, as you well know, Galen compounds of sulphur, pepper, and mustard-seed in given quantities. Heat then must have thickened the air of the chamber, by driving off the more subtle parts which some proclaim (*chante*) as so impossible. I already perceive that to elude the force of so many reasons and experiments, I shall be told that the instances I have produced, may indeed be proved in our gross and impure air, but that it would be otherwise in pure air if such can be found in nature: and, certainly I desire nothing better to induce me to cry victory. For what? do they believe that I imagine the Sieur Brun and the rest, who have found the increase of weight in question, have obtained a purer air by bills of exchange from beyond the confines of nature?

(*To be continued.*)

ART. VIII. *A further Inquiry into the Nature of the Products of the slow Combustion of Ether.* *By* J. F. Daniell, F.R.S. *and* M.R.I.

SHORTLY after the publication of my first paper * upon the acid discovered by Sir Humphry Davy during his researches into the nature of flame, it was suggested to me by my friend Mr. Richard Phillips, that most of the properties of the salts described by me, as well as the equivalent number of the acid derived from their analysis, agreed very closely with those of the acetic acid. Although I must own that at the time I thought the evidence sufficiently conclusive against their identity, yet with the promise of his assistance I willingly undertook a further series of experiments upon the subject. The results

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* Journal, Vol. VI. p. 318.

I shall now as concisely as possible describe, as being in themselves interesting, and as furnishing rather a curious anecdote of the progress of the investigation.

I prepared a large quantity of the acid from sulphuric ether in the manner described in my former paper, making use of a lamp with three burners, which greatly accelerated the operation. The first product I rectified by drawing off from it at a low heat one-third of the quantity. This I did, to get rid of any ether with which it might have been contaminated, to the influence of which Mr. Phillips seemed to think some of the singular results which I had before obtained, might have been owing.

The liquor condensed from this distillation had not the slightest smell of sulphuric ether. It was inflammable, and burned with a pale blue flame, leaving but a small tasteless fluid residue. The *spirit* had a pungent, suffocating smell, and its taste was hot and rather acrid. It did not change the colour of litmus. It evaporated without leaving any residue, with a very gentle heat. Neither water, alcohol, nor ether produced any change in its appearance. When dropped into the solutions of muriate of gold and nitrate of silver, and warmed, it produced no precipitation of those metals.

The *acid* thus purified was intensely sour, of a peculiar pungent smell, which when received into the lungs, produced great irritation. Some pure acetic acid was procured, with which a small quantity of sulphuric ether was mixed, and with the two the following comparative experiments were made, with the assistance of Mr. Phillips in his laboratory:

Exp. 1.—Some solution of the muriate of gold was put into a test-tube, with a small quantity of the acid: upon the application of a very gentle heat the mixture became turbid, and appeared green by transmitted light. The gold was instantaneously reduced, and completely coated the inside of the glass. Some of the mixture of acetic acid and ether was likewise dropped into muriate of gold, but no change ensued even when strongly heated.

Exp. 2.—A little of the acid mixed with some solution of nitrate of silver produced an instantaneous turbidness. The mixture appeared blue by transmitted light, and the tube in which the experiment was made, was shortly lined with metallic silver. The mixture of acetic acid and ether produced no effect upon some of the same solution.

Exp. 3.—Some of the acid was dropped into a solution of muriate of platinum. When heated no change ensued. Upon examining the muriate of platinum it was found to be very acid. The excess was carefully neutralized with soda, when upon treating it as before a violent action took place, some of the liquid was thrown out of the tube, and the whole turned intensely black. Upon standing at rest for a few minutes the tube became coated with metallic particles of platinum, interspersed with a black powder not of a metallic appearance. The acetic acid and ether produced no effect upon the neutral solution.

Exp. 4.—Some of the acid was neutralized with carbonate of barytes: the solution was set to evaporate with a very gentle heat, but it underwent a certain degree of decomposition as it became very brown and pasty, and no crystals were obtained from it. For the sake of preserving the acid, the barytes was very carefully precipitated by dilute sulphuric acid. The sulphate of barytes was separated by the filter. After this process, the recovered acid still retained its property of reducing the metallic salts.

Exp. 5.—The acid was saturated with potash, and furnished, by very careful evaporation, long prismatic transparent crystals. The solution of this salt reduced the metallic solution with as much ease as the pure acid. No such effect was produced by acetate of potash.

Exp. 6.—Some peroxide of mercury, prepared from nitric acid, was put into a glass with some of the acid, and gently warmed. A very bulky white salt was instantly formed, which appeared to be nearly, if not wholly, insoluble in water. An equal quantity of the peroxide was treated in the same way with the mixture of acetic acid and ether, and a perfectly transparent colourless solution resulted, which was not at all altered by heat.

Exp. 7.—A quantity of peracetate of mercury was formed by dissolving peroxide of mercury, prepared from nitric acid, in acetic acid. Into a portion of this solution a small quantity of the acid was dropped, and, upon heating the mixture, an instantaneous precipitation of the bulky white salt obtained in Exp. 6 took place, in such a quantity that the whole mixture became solid, and the glass tube in which the experiment was made was upset without spilling a drop. It is hardly necessary to mention, that no such effect was produced by the mixture of acetic acid and ether.

After these experiments which were often repeated and varied in ways which it is unnecessary to detail, we thought that the distinct and peculiar nature of the acid formed during the slow combustion of ether was sufficiently established, and Mr. Phillips kindly requests me to state that he had no longer a doubt existing in his mind.

By a curious chance, I had no sooner satisfied others with regard to this subject, than I had reason to entertain doubts myself upon this very point. Being engaged in following up another branch of the subject in my own laboratory, (the object of our joint inquiry being considered accomplished,) I had occasion to prepare some peracetate of mercury. For this purpose I took some peroxide of mercury *per se*, and put it into acetic acid in a gentle heat. To my great surprise instead of dissolving entirely as the *nitrous* red oxide had always done, a large quantity of white insoluble salt was formed, exactly resembling that obtained in former experiments by the acid from ether.

Something of the true explanation of these apparently contradictory results soon occurred to me, and I immediately set about the following experiment to elucidate the matter.

Exp. 8.—Some protoxide of mercury was put into acetic acid and warmed, a spongy white salt of a silvery micaceous appearance was speedily formed, which was nearly insoluble in cold water. Some of the same protoxide was treated with the acid from ether, and an exactly similar salt resulted. Both products were carefully dried upon filtering paper, and set apart. At the end of a few days they were examined : the first was wholly unaltered in its appearance, but the second had assumed a grey hue, and was found upon examination with a lens, to be full of minute globules of revived mercury.

From hence it would appear that the acid formed during the slow combustion of ether is in fact the acetic, but combined with some substance of a highly disoxygenising nature, different from ether. In this manner we can account for the rapid reduction of the metallic oxides in the former experiments, and also for the instantaneous change of the soluble peracetate of mercury into the insoluble proto-acetate. In the latter case it is clear that the disoxygenating substance (whatever its nature may be,) took from the oxide of mercury in the peracetate one proportion of its oxygen, and precipitated the salt in the form of proto-acetate.

Upon this view of the subject it would seem to follow that the peroxide of mercury formed *per se* differed from the peroxide formed from nitric acid, in containing either combined or mixed a portion of protoxide. To bring this idea to the proof, I proceeded as follows.

Exp. 9.—Fifty grains of the *nitric* peroxide of mercury (red precipitate,) were put into pure muriatic acid. The whole dissolved readily, except a slight cloudiness. Fifty grains of the peroxide *per se* were treated in the same manner. A considerable quantity of grey insoluble matter remained, which being separated weighed 0.7 grains. It consisted chiefly of metallic mercury in a state of minute division, mixed with a very small portion of calomel. The quantity of this latter indicated, contrary to my expectation, but a very minute proportion of protoxide, but it is obvious that the metallic mercury during the process of solution in the acetic acid, combined with one proportion of the oxygen of the peroxide, forming thereby protoxide both by composition and decomposition. Why this should happen during the action of the acetic, and not during the action of the muriatic acid, is not perhaps at the first view of the subject quite clear, but I think upon consideration may be satisfactorily explained. In the former, a predisposing affinity exists between all the ingredients obviously tending to such an arrangement, resulting from the combined attraction of the mercury for oxygen, and of the acetic acid for the protoxide, but in the latter no such predisposing force can exist. When muriatic acid is put to the mixture of mercury and peroxide, a double decomposition takes place; the two proportions of oxygen in the oxide combine with two proportions of hydrogen derived from the acid, and water and a bichloride are formed, the metallic mercury remaining untouched: for, although we can readily enough conceive in the former instance that the play of affinities should produce the permanent arrangement of the proto-acetate of mercury, it is impossible to imagine that any predisposing affinity should determine the composition of protoxide of mercury, and at the same time its decomposition, as must have been the case if protochloride of mercury had been formed from the mixture of metal and peroxide; that is to say, protoxide of mercury must have been formed by the action of the metallic mercury upon the peroxide, merely for the purpose of being again decomposed by the agency of the acid, the whole arrangement being determined by the affinity of two substances which in point of fact did not exist.

I believe that it is the general opinion that the red precipitate is a more impure preparation of the peroxide of mercury than the oxide *per se;* but these experiments would seem to prove the contrary. Certain it is, that in medicine the former, which is of very low price, is not considered to answer the same purposes as the latter, which is a costly drug. Might not a due mixture of metallic mercury with the former assimilate the properties of the two? It is right to mention that the oxides which I employed were obtained from Messrs. Allen and Co. Plough-court, the accuracy of whose preparations are well known.

But to return to the immediate object of this paper. I endeavoured in various ways to separate the acetic acid, formed during the slow combustion of sulphuric ether, from the substance with which it appeared to be combined, so as to produce the latter in an isolated state, but without success. It appeared to undergo such rapid decomposition when exposed to but a moderate heat, that, combined at the same time with the speedy decomposition of most of the acetates, I could not effect my purpose. The salts which I obtained in a crystallized form held this substance in combination with them, so that their solution produced the same effects as the acid of which they were composed. It is owing to this circumstance that most of the salts, as I described in my former paper, burned with flame, and afterwards glowed like a live coal, and the results of the analysis which I made of the acid by means of the salt of barytes and chlorate of potash are explained, and the source of the excess of hydrogen clearly demonstrated. Thus the very same causes seemed to fix distinctive characters upon the lampic acid as contributed so long to separate the pyroligneous, the formic, and other modifications of the acetic acid.

Notwithstanding the failure of my endeavours to separate the substance of which I was in search, from its combinations I am enabled to throw considerable light upon its nature and formation by the discovery of an analogous compound, which is easily obtained in an isolated form. This compound is produced by the slow combustion of nitric ether.

Exp. 10.—When nitric ether is substituted for sulphuric in the aphlogistic lamp, the platinum wire glows as readily, but the fumes of the former are of a denser nature than those of the latter. They possess a more suffocating and more disagreeable smell, and are more inflammable, so that greater caution is requisite in adjusting the wire to prevent their breaking out into flame. After the process has been carried on a short time with the apparatus described in my former paper, an incrustation may be observed forming about the wire, a considerable quantity of liquid is condensed in the receiver; and by continuing the process, around the mouth, and upon the top of the alembic-head, is deposited a resinous-looking substance of a bright yellow colour. These products were collected and examined as follows:—

Exp. 11.—The liquid was purified from any spirituous or etherial mixture by careful distillation. The fluid condensed from this rectification was colourless, inflammable, and of a pungent very disagreeable smell. It was not affected by mixture with water, or alcohol, and did not affect the colour of litmus. The residual liquor was of a bright yellow colour, of a hot, acrid, and nauseous taste, and turned litmus paper red. Its smell was suffocating, and very unpleasant. It effected the reduction of the metals from their solutions in the same way as the lampic acid, but even more speedily. It instantly converted per-acetate of mercury into proto-acetate.

Exp. 12.—The resinous substance was highly inflammable. It burned rapidly with a hissing noise and scintillation, leaving a very bulky coal, such as is formed by the decomposition of a quill or other animal matter. It was soluble both in water and alcohol, but very sparingly in ether. Its taste was bitter, astringent, and very unpleasant.

Exp. 13.—The solutions of this matter produced the same effect upon the metallic salts as the distilled liquid; and when the acid of the latter was neutralised by an alkali, much of the resinous substance was obtained by gentle evaporation.

Exp. 14.—Its vapour, when heated, smelled very strongly of ammonia. A small piece of it was placed in a glass tube with a piece of turmeric paper, and upon the application of heat, the colour of the paper was turned to red.

Exp. 15.—It did not burn out of the contact of oxygen.

Exp. 16.—With the assistance of Mr. Faraday, I attempted to analyse this curious and interesting substance by means of per-oxide of copper. A quantity, weighing 1.8 grains, was taken, which was at first rather tough and tenacious. Being exposed to a very gentle heat, it lost in weight 0.2 grains, and became more hard and brittle. It was then triturated in a mortar, and well mixed with a large quantity of per-oxide of copper, and exposed to a red heat in an iron tube. The products of the decomposition were received over mercury. The quantity of gaseous matter, with all corrections made, amounted to 7.37 cubic inches. Upon examining the oxide of copper after the operation, it appeared that some of the carbon had escaped decomposition. It was, therefore, again triturated and mixed in a mortar, and returned into the tube. A further quantity of gas was thus obtained, amounting to 0.32 cubic inch. This added to the former produce, made the whole 7.69 cubic inches. This gaseous matter could not be fired with hydrogen, and produced no change in nitric oxide. Lime water was copiously precipitated by it. After these preliminary trials it was found that 6.23 cubic inches were absorbed by potash; and the residue, 1.46 cubic inch, possessed all the negative properties of azote. Thus the calculated results of the analysis stand

	Grs.	
6.23 inches carbonic acid =	0.79	carbon
1.46 ditto azote . . . =	0.43	azote
Loss . . . =	0.38	hydrogen
	1.60	

Now, the nearest definite proportions to these would be

Grs.	
0.75	carbon
0.43	azote
0.34	hydrogen
1.52	

Or upon the scale of equivalents,

4 proportions carbon	3.00
1 ditto azote	1.75
11 hydrogen	1.37

Which are further equivalent to

4 proportions of sub-carburetted hydrogen,	4.00
1 proportion of ammonia	2.12

To this singular compound, possessed of such distinctive characters, it will, no doubt, be deemed right to give a name, and I will venture to suggest that *Hydro-carburet of Azote*, will not inappropriately express its composition.

Exp. 17.—Some solution of muriate of platinum was taken, in which all excess of acid was neutralized carefully by soda. When the solution of hydro-carburet of azote was added to it, and gently warmed, a violent action, almost amounting to an explosion, took place; much of the mixture was thrown about, and a very black precipitate was formed. This consisted of metallic platinum, mixed with a large quantity of a deep black powder. The latter was separated on a filter, and gently warmed, to dry it. It had scarce parted with its moisture when it exploded with flame and noise, and nothing was left but reduced platinum.

Exp. 18.—A small lamp trimmed with nitric ether with the wire glowing, was placed under a bell-glass full of atmospheric air, and surrounded with water. A great absorption took place, amounting to one-fourth, when the lamp was extinguished. Upon examining the residual air and the water, the former was found to contain a large proportion of nitric oxide, and the latter of nitric acid. When nitric ether alone was placed in the same situation, an absorption, after some time, took place; and the water contained nitric acid, but no nitric oxide was formed. These experiments were repeated, with the same results, after having put some pieces of potash into the ether.

It was with a view of throwing some light upon the changes produced in the air by the slow combustion effected by the aphlogistic lamp, that the last experiments were instituted; but as this inquiry has led me into a wider field than I had at first anticipated, and in which I am still engaged, I shall reserve this further discussion for a future communication, and shall conclude this paper with a brief recapitulation of the principal facts established.

It appears, then, that the acid formed during the slow combustion of ether is the acetic, but combined with some compound of carbon and hydrogen, differing from ether or alcohol. In the product from sulphuric ether, it is less volatile, and more energetic in its action upon oxygenated bodies than ether. Owing to its easy decomposition, its point of volatilization, and ignorance of its peculiar affinities, it has not yet been separated from its concomitants, and exhibited in a distinct form.

In the products from nitric ether, this compound proves to be a triple combination of carbon, hydrogen, and azote, hitherto unknown. Its effects upon metallic oxides appear to be quite analogous to those of the compound produced from sulphuric ether, but it is easily separable from the other products. It forms a fulminating combination with platinum.

ART. IX.—*On some new Electro-Magnetical Motions, and on the Theory of Magnetism.* *By* M. Faraday, *Chemical Assistant in the Royal Institution.*

In making an experiment the beginning of last week, to ascertain the position of the magnetic needle to the connecting wire of a voltaic apparatus, I was led into a series which appear to me to give some new views of electro-magnetic action, and of magnetism altogether; and to render more distinct and clear those already taken. After the great men who have already experimented on the subject, I should have felt doubtful that any thing I could do could be new or possess an interest, but that the experiments seem to me to reconcile considerably the opposite opinions that are entertained on it. I am induced in consequence to publish this account of them, in the hope they will assist in making this important branch of knowledge more perfect.

The apparatus used was that invented by Dr. Hare of Philadelphia, and called by him a calorimotor; it is in fact a single pair of large plates, each having its power heightened by the induction of others. Consequently all the positions and motions of the needles, poles, *&c.*, are opposite to those produced by an apparatus of several plates; for, if a current be supposed to exist in the connecting wire of a battery from

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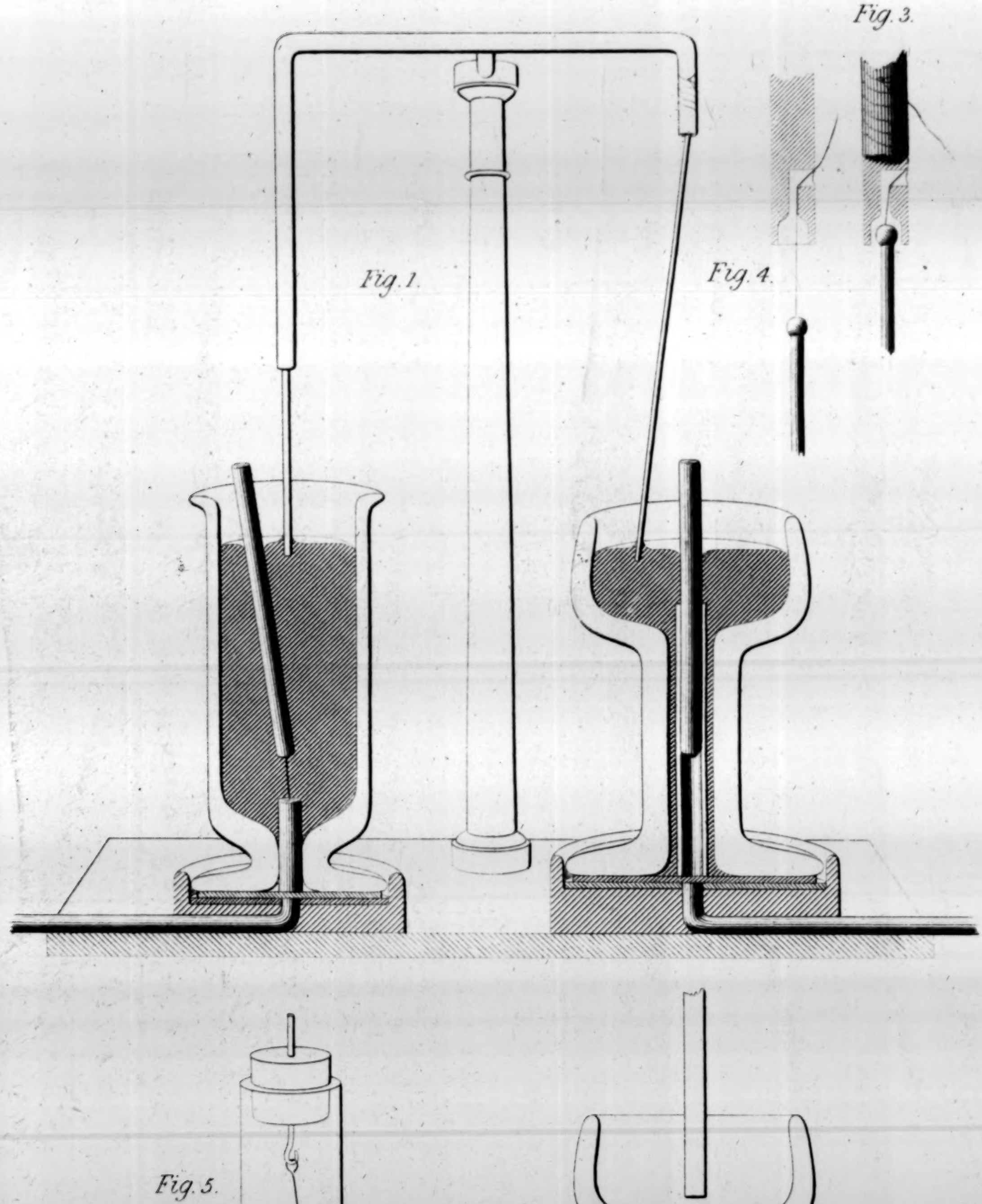

Fig. 1.

Fig. 3.

Fig. 4.

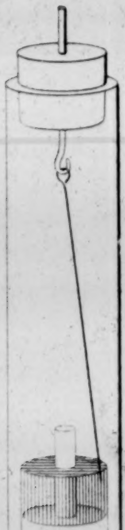

Fig. 5.

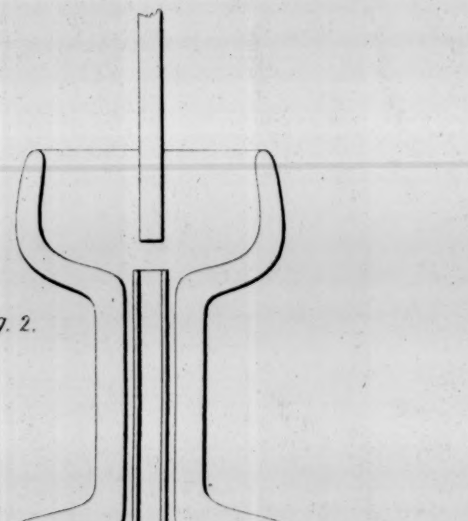

Fig. 2.

A. A. del.

J. B. Taylor sc.

the zinc to the copper, it will be in each connected pair of plates from the copper to the zinc; and the wire I have used is that connection between the two plates of one pair. In the diagrams I may have occasion to subjoin the ends of the connecting wire, marked Z and C, are connected with the zinc and copper-plates respectively; the sections are all horizontal and seen from above, and the arrow-heads have been used sometimes to mark the pole of a needle or magnet which points to the north, and sometimes to mark the direction of motion; no difficulty can occur in ascertaining to which of those uses any particular head is applied.

On placing the wire perpendicularly, and bringing a needle towards it to ascertain the attractive and repulsive positions with regard to the wire; instead of finding these to be four, one attractive and one repulsive, for each pole, I found them to be eight, two attractive and two repulsive for each pole: thus allowing the needle to take its natural position across the wire, which is exactly opposite to that pointed out by Oersted for the reason before-mentioned, and then drawing the support away from the wire slowly, so as to bring the north pole, for instance, nearer to it, there is attraction, as is to be expected; but on continuing to make the end of the needle come nearer to the wire, repulsion takes place, though the wire still be on the same side of the needle. If the wire be on the other side of the same pole of the needle, it will repel it when opposite to most parts between the centre of motion and the end; but there is a small portion at the end where it attracts it. Fig. 1, plate iii, shews the positions of attraction for the north and south poles, fig. 2, the positions of repulsion.

If the wire be made to approach perpendicularly towards one pole of the needle, the pole will pass off on one side, in that direction which the attraction and repulsion at the extreme point of the pole would give; but, if the wire be continually made to approach the centre of motion, by either the one or other side of the needle, the tendency to move in the former direction diminishes; it then becomes null, and the needle is quite indifferent to the wire; and ultimately the motion is reversed, and the needle powerfully endeavours to pass the opposite way.

It is evident from this that the centre of the active portion of either limb of the needle, or the true pole, as it may be called, is not at the extremity of the needle, but may be represented by a point generally in the axis of the needle, at some little distance from the end. It was evident, also, that this point had a tendency to revolve round the wire, and necessarily, therefore, the wire round this point; and as the same effects in the opposite direction took place with the other pole, it was evident that each pole had the power of acting on the wire by itself, and not as any part of the needle, or as connected with the opposite pole.

By attending to fig. 3, which represents sections of the wire in its different positions to the needle, all this will be plain; the active poles are represented by two dots, and the arrow-heads shew the tendency of the wire in its positions to go round these poles.

Several important conclusions flow from these facts; such as that there is no attraction between the wire and either pole of a magnet; that the wire ought to revolve round a magnetic pole, and a magnetic pole round the wire; that both attraction and repulsion of connecting wires, and probably magnets, are compound actions; that true magnetic poles are centres of action induced by the whole bar, *&c. &c.* Such of these as I have been able to confirm by experiment, shall be stated, with their proofs.

The revolution of the wire and the pole round each other being the first important thing required to prove the nature of the force mutually exerted by them, various means were tried to succeed in producing it. The difficulty consisted in making a suspension of part of the wire sufficiently delicate for the motion, and yet affording sufficient mass of matter for contact. This was overcome in the following manner:—A piece of brass wire had a small button of silver soldered on to its end, a little cup was hollowed in the silver, and the metal being amalgamated, it would then retain a drop of mercury in it, though placed upside down for an upper centre of motion; for a lower centre, a similar cup was made of copper, into which a little mercury was put; this was placed in a jar of water under the former centre. A piece of copper wire was then bent into the form of a crank, its ends amalgamated, and the distances being arranged, they were placed in the cups. To prevent too much friction from the weight of the wire on the lower cup, it had been passed through a cork duly adjusted in size, and that being pushed down on the wire till immersed in the water, the friction became very little, and the wire very mobile yet with good contacts. The plates being then connected with the two cups, the apparatus was completed. In this state, a magnetic pole being brought to the centre of motion of the crank, the wire immediately made an effort to revolve until it struck the magnet, and that being rapidly brought round to the other side, the wire again made a revolution, giving evidence that it would have gone round continually but for the extension of the magnet on the outside. To do away with this impediment, the wire and lower metal cup were removed, and a deep basin of mercury placed beneath; at the bottom of this was a piece of wax, and a small round bar magnet was stuck upright in it, so that one pole was about half or three-fourths of an inch above the surface of the mercury, and directly under the silver cup. A straight piece of copper wire, long enough to reach from the cup, and dip about half an inch into the mercury, had its ends amalgamated, and a small round piece of cork fixed on to one of them to make it more buoyant; this being dipped in the mercury close beside the magnet, and the other end placed under the little cup, the wire remained upright, for the adhesion of the cork to the magnet was sufficient for that purpose, and yet at its lower end had freedom of motion round the pole. The connection being now made from the plates to the upper cup, and to the mercury below, the wire immediately began to revolve round the pole of the magnet, and continued to do so as long as the connexion was continued.

When it was wished to give a large diameter to the circle described by the wire, the cork was moved from the magnet, and a little loop of platinum passed round the magnet and wire, to prevent them from separating too far. Revolution again took place on making the connexion, but more slowly as the distance increased.

The direction in which the wire moved was according to the way in which the connexions were made, and to the magnetic pole brought into action. When the upper part of the wire was connected with the zinc, and the lower with the copper plate, the motion round the north and south poles of a magnet were as in fig. 4 and 5, looking from above; when the connexions were reversed, the motions were in the opposite direction.

On bringing the magnetic pole from the centre of motion to the side of the wire, there was neither attraction nor repulsion; but the wire endeavoured to pass off in a circle, still having the pole for its centre, and that either to the one side or the other, according to the above law.

When the pole was on the outside the wire, the wire moved in a direction directly contrary to that taken when the pole was in the inside; but it did not move far, the endeavour was still to go round the pole as a centre, and it only moved till that power and the power which retained it in a circle about its own axis were equipoised.

The next object was to make the magnet revolve round the wire. This was done by so loading one pole of the small magnet with platinum that the magnet would float upright in a basin of mercury, with the other pole above its surface; then connecting the mercury with one plate, and bringing a wire from the other perpendicularly into it in another part near the floating magnet: the upper pole immediately began to revolve round the wire, whilst the lower pole being removed away caused no interference or counteracting effect.

The motions were again according to the pole and the connexions. When the upper part of the wire was in contact with the zinc plate, and the lower with the copper, the direction of the curve described by the north and south poles were as in fig. 6 and 7. When the connexions were reversed, the motions were in the opposite directions.

Having succeeded thus far, I endeavoured to make a wire and a magnet revolve on their own axis by preventing the rotation in a circle round them, but have not been able to get the slightest indications that such can be the case; nor does it, on consideration, appear probable. The motions evidently belong to the current, or whatever else it be, that is passing through the wire, and not to the wire itself, except as the vehicle of the current. When that current is made a curve by the form of the wire, it is easy to conceive how, in revolving, it should take the wire with it; but when the wire is straight, the current may revolve without any motion being communicated to the wire through which it passes.

M. Ampere has shewn that two similar connecting wires, by which is meant, having currents in the same direction through them, attract each other; and that two wires having currents in opposite directions through them, repel each other; the attraction and repulsion taking place in right lines between them. From the attraction of the north pole of a needle on one side the wire and of the south on the other, and the repulsion of the poles on the opposite sides, Dr. Wollaston called this magnetism vertiginous, and conceived that the phænomena might be explained upon the supposition of an electro-magnetic current passing round the axis of the conjunctive wire, its direction depending upon that of the electric current, and exhibiting north and south powers on the opposite sides. It is, indeed, an ascertained fact, that the connecting wire has different powers at its opposite sides; or rather each power continues all round the wire, the direction being the same; and hence it is evident that the attractions and repulsions of M. Ampere's wires are not simple, but complicated results.

A simple case which may be taken of magnetic motion, is the circle described by the wire or the pole round each other. If a wire be made into a helix, as M. Ampere describes, the arrangement is such that all the vertiginous magnetism, as Dr. Wollaston has named it, of the one kind, or one side of the wire, is concentrated in the axis of the helix, whilst the contrary kind is very much diffused, *i. e.*, the power exerted by a great length of wire to make a pole pass one way round it, all tends to carry that pole to a particular spot, whilst the opposite power is diffused and much weakened in its action on any one pole. Hence the power on one side of the wire is very much concentrated, and its particular effects brought out strongly, whilst that on the other is rendered insensible. A means is thus obtained of separating, as it were, the one power from the other: but when this is done, and we examine the end of the helix, it is found very much to resemble a magnetic pole; the power is concentrated at the extremity of the helix; it attracts or repels one pole in all directions; and I find that it causes the revolution of the connecting wire round it, just as a magnetic pole does. Hence it may, for the present, be considered identical with a magnetic pole; and I think that the experimental evidence of the ensuing pages will much strengthen that opinion.

Assuming, then, that the pole of a magnetic needle presents us with the properties of one side of the wire, the phenomena it presents with the wire itself, offers us a means of analysis which, probably, if well pursued, will give us a much more intimate knowledge of the state of the powers active in magnets. When it is placed near the wire, always assuming the latter to be connected with the battery, it is made to revolve round it, passing towards that side by which it is attracted, and from that side by which it is repelled, *i. e.*, the pole is at once attracted and repelled by equal powers, and therefore neither recedes or approaches; but the powers being from opposite sides of the wire, the pole in its double effort to recede from one side and approach the other revolves in the circle, that circle being evidently decided by the particular pole and state of the wire, and deducible from the law before mentioned.

The phenomena presented by the approximation of one pole to two or more wires, or two poles to one or more wires, offer many illustrations of this double action, and will lead to more correct views of the magnet. These experiments are easily made by loading a needle with platinum at one pole, that the other may float above mercury, or by almost floating a small magnetic needle by cork in a basin of water, at the bottom of which is some mercury with which to connect the wires. In describing them I shall refrain from entering into all their variations, or pursuing them to such conclusions as are not directly important.

Two similar wires, Ampere has shewn, attract each other; and Sir H. Davy has shewn that the filings adhering to them attract from one to another on the same side. They are in that position in which the north and south influence of the different wires attract each other. They seem also to neutralize each other in the parts that face, for the magnetic pole is quite inactive between them, but if put close together, it moves round the outside of both, circulating round them as round one wire, and their influences being in the same direction, the greatest effect is found to be at the farther outside surfaces of the wires. If several similar wires be put together, side by side like a ribbon, the result is the same, and the needle revolves round them all; the internal wires appear to lose part of their force, which is carried on towards the extreme wire in opposite directions, so that the floating pole is accelerated in its motion as it passes by the edges that they form. If, in place of a ribbon of parallel wires, a slip of metal be used, the effect is the same, and the edges act as if they contained in a concentrated state the power that belonged to the inner portion of the slip. In this way we procure the means of removing, as it were, in that direction, the two sides of the wire from each other.

If two wires in opposite states be arranged parallel to each other, and the pole be brought near them, it will circulate round either of them in obedience to the law laid down; but as the wires have opposite currents, it moves in opposite directions round the two, so that when equidistant from them, the pole is propelled in a right line perpendicular to the line which joins them, either receding or approaching; and if it approaches, passing between and then receding: hence it exhibits the curious appearance of being first attracted by the two wires, and afterwards repelled. (Fig. 8.) If the connexion with both wires be inverted, or if the pole be changed, the line it describes is in the opposite direction. If these two opposite currents be made by bending a piece of silked wire parallel to itself, fig. 9, it, when connected with the apparatus, becomes a curious magnet; with the north pole, for instance, it attracts powerfully on one side at the line between the two currents, but repels strongly to the right or left; whilst on the other side the line repels the north pole, but attracts it strongly to the right or left. With the south pole the attractions and repulsions are reversed.

When both poles of the needle were allowed to come into action on the wire or wires, the effects were in accordance with those described. When a magnetic needle was floated on water, and the perpendicular wire brought towards it, the needle turned round more or less, until it took a direction perpendicular to, and across the wire, the poles being in such positions that either of them alone would revolve round the wire in a circle proceeding by the side to which it had gone, according to the law before stated. The needle then approaches to the wire, its centre (not either pole) going in a direct line towards it. If the wire be then lifted up and put down on the other side the needle, the needle passes on in the same line receding from the wire, so that the wire seems here to be both attractive and repulsive of the needle. This effect will be readily understood from fig. 10, where the poles and direction of the wire is not marked, because they are the same as before. If either be reversed, the others reverse themselves. The experiment is analogous to the one described above; there the pole passed between two dissimilar wires, here the wire between two dissimilar poles.

If two dissimilar wires be used, and the magnet have both poles active, it is repelled, turned round, or is attracted in various ways, until it settles across between the two wires; all its motions being easily reducible to those impressed on the poles by the wires, both wires and both poles being active in giving that position. Then if it happens not to be midway between the two, or they are not of equal power, it goes slowly towards one of them, and acts with it just as with the single wire of the last paragraph.

Fig. 11 and 12 exhibit more distinctly the direction of the forces which influence the poles in passing between two dissimilar wires: fig. 11, when the pole draws up between the wires; fig. 12, the pole thrown out from between them. The poles and state of the wire are not marked, because the diagrams illustrate the attraction and repulsion of both poles: for any particular pole, the connexion of the wires must be accordingly.

If one of the poles be brought purposely near either wire in the position in which it appears to attract most strongly, still if freedom of motion be given by a little tapping, the needle will slip along till it stands midway across the wire.

A beautiful little apparatus has been made by M. de la Rive, to whom I am indebted for one of them, consisting of a small voltaic combination floating by a cork; the ends of the little zinc and copper slips come through the cork, and are connected above by a piece of silked wire which has been wrapped four or five times round a cylinder, and the wires tied together with a silk thread so as to form a close helix about one inch in diameter. When placed on acidulated water it is very obedient to the magnet, and serves admirably to transform, as it were, the experiments with straight wires that have been mentioned, to the similar ones made with helices. Thus, if a magnet be brought near it and level with its axis, the apparatus will recede or turn round until that side of the curve next to the nearest pole is the side attracted by it. It will then approach the pole, pass it, recede from it until it gains the middle of the magnet, where it will rest like an equator round it, its motions and position being still the same as those before pointed out. (Fig. 13.) If brought near either pole it will still return to the centre; and if purposely placed in the opposite direction at the centre of the magnet, it will pass off by either pole to which it happens to be nearest, being apparently first attracted by the pole and afterwards repelled, as is actually the case; will, if any circumstance disturbs its perpendicularity to the magnet, turn half way round; and will then pass on to the magnet again, into the position first described. If, instead of passing the magnet through the curve, it be held over it; it stands in a plane perpendicular to the magnet, but in an opposite direction to the former one. So that a magnet, both within and without this curve, causes it to direct.

When the poles of the magnet are brought over this floating curve, there are some movements and positions which at first appears anomalous, but are by a little attention easily reducible to the circular movement of the wire about the pole. I do not think it necessary to state them particularly.

The attractive and repulsive positions of this curve may be seen by fig. 13, the curve in the two dotted positions is attracted by the poles near them. If the positions be reversed, repulsion takes place.

From the central situation of the magnet in these experiments, it may be concluded that a strong and powerful curve or helix would suspend a powerful needle in its centre. By making a needle almost float on water aud putting the helix over a glass tube, this result has in part been obtained.

In all these magnetic movements between wires and poles, those which resemble attraction and repulsion, that is to say, those which took place in right lines, required at least either two poles and a wire, or two wires and a pole; for such as appear to exist between the wire and either pole of the battery, are deceptive and may be resolved into the circular motion. It has been allowed, I believe, by all who have experimented on these phenomena, that the similar powers repel and the dissimilar powers attract each other; and that, whether they exist in the poles of magnets or in the opposite sides of conducting wires. This being admitted, the simplest cases of magnetic action will be those exerted by the poles of helices, for, as they offer the magnetic states of the opposite sides of the wire independent, or nearly so, one of the other, we are enabled by them to bring into action two of those powers only, to the exclusion of the rest; and, from experiment it appears that when the powers are similar, repulsion takes place, and when dissimilar, attraction; so that two cases of repulsion and one of attraction are produced by the combination of these magnetic powers *.

The next cases of magnetic motion, in the order of simplicity, are those where three powers are concerned or those produced by a pole and a wire. These are the circular motions described in the early part of this paper. They resolve themselves into two, a north pole and the wire round each other, and a south pole, and the wire round each other. The law which governs these motions has been stated.

Then follow the actions between two wires, these when similarly electrified attract as M. Ampere has shewn; for then the opposite sides are towards each other, and the four powers all combine to draw the currents together forming a double attraction; but, when the wires are dissimilar they repel, because, then on both sides the wire the same powers are opposed, and cause a double repulsion.

The motions that result from the action of two dissimilar poles and a wire next follow: the wire endeavours to describe opposite circles round the poles; consequently, it is carried in a line passing through the central part of the needle in which they are situated. If the wire is on the side on which the circles close together, it is attracted; if on the opposite side, from whence the circles open, it is repelled, fig. 10.

The motions of a pole with two wires are almost the same as the last; when the wires are dissimilar, the pole endeavours to form two opposite circles about the wires; when it is on that side the wires on which the circles meet, it is attracted; when on the side on which they open, it is repelled, figs. 8, 11, 12.

Finally, the motion between two poles and two dissimilar wires, is an instance where several powers combine to produce an effect.

M. Ampere, whilst reasoning on the discovery of M. Oersted, was led to the adoption of a theory, by which he endeavoured to account for the properties of magnets, by the existence of

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* This is perhaps not strictly true, because, though the opposite powers are weakened, they still remain in action.

concentric currents of electricity in them, arranged round the axis of the magnet. In support of this theory, he first formed the spiral or helix wire, in which currents could be made to pass nearly perpendicular to, and round the axis of a cylinder. The ends of such helices were found, when connected with the voltaic apparatus to be in opposite magnetic states, and to present the appearance of poles. Whilst pursuing the mutual action of poles and wires, and tracing out the circular movements, it seemed to me that much information respecting the competency of this theory might be gained from an attempt to trace the action of the helix, and compare it with that of the magnet more rigorously than had yet been done; and to form artificial electro-magnets, and analyze natural ones. In doing this, I think I have so far succeeded as to trace the action of an electro-magnetic pole, either in attracting or repelling, to the circulating motion before described.

If three inches of connecting wire be taken, and a magnetic pole be allowed to circulate round the middle of it, describing a circle of a little less than one inch in diameter, it will be moved with equal force in all parts of the circle, fig. 14; bend then the wire into a circle, leaving that part round which the pole revolves perpendicularly, undisturbed, as seen by the dotted lines, and make it a condition that the pole be restrained from moving out of the circle by a radius. It will immediately be evident that the wire now acts very differently on the pole in the different parts of the circle it describes. Every part of it will be active at the same time on the pole, to make it move through the centre of the wire ring, whilst as it passes away from that position the powers diverge from it, and it is either removed from their action or submitted to opposing ones, until on its arriving at the opposite part of the circle it is urged by a very small portion indeed of those which moved it before. As it continues to go round, its motion is accelerated, the forces rapidly gather together on it, until it again reaches the centre of the wire ring where they are at their highest, and afterwards diminish as before. Thus the pole is perpetually urged in a circle, but with powers constantly changing. If the wire ring be conceived to be occupied by a plane, then the centre of that plane is the spot where the powers are most active on the pole, and move it with most force. Now this spot is actually the pole of this magnetic apparatus. It seems to have powers over the circulating pole, making it approach or attracting it on the one side, and making it recede or repelling it on the other, with powers varying as the distance; but its powers are only apparent, for the force is in the ring, and this spot is merely the place where they are most accumulated: and though it seems to have opposite powers, namely, those of attracting and repelling; yet this is merely a consequence of its situation in the circle, the motion being uniform in its direction, and really and truly impressed on the pole by its motor, the wire.

At page 81, it was shewn that two or more similar wires put together in a line, acted as one; the power being, as it were, accumulated towards the extreme wires, by a species of induction taking place among them all: and at the same time was noticed the similar case of a plate of metal connecting the ends of the apparatus, its powers being apparently strongest at the edges. If, then, a series of concentric rings be placed one inside the other, they having the electric current sent through them in the same direction; or if, which is the same thing, a flat spiral of silked wire passing from the centre to the circumference be formed, and its ends be in connexion with the battery, fig. 15, then the circle of revolution would still be as in fig. 14, passing through the centre of the rings or spiral, but the power would be very much increased. Such a spiral, when made, beautifully illustrates this fact; it takes up an enormous quantity of iron filings, which approach to the form of cones, so strong is the action at the centre; and its action on the needle by the different sides, is eminently powerful.

If in place of putting ring within ring, they be placed side by side, so as to form a cylinder, or if a helix be made, then the same kind of neutralization takes place in the intermediate wires, and accumulated effect in the extreme ones, as before. The line which the pole would now travel, supposing the inner end of the radius to move over the inner and outer surface of the cylinder, would be through the axis of the cylinder round the edge to one side, back up that side, and round to the axis, down which it would go, as before. In this case the force would probably be greatest at the two extremes of the axis of the cylinder, and least at the middle distance on the outside.

Now consider the internal space of the cylinder filled up by rings or spirals, all having the currents in the same direction: the direction and kind of force would be the same, but very much strengthened: it would exist in the strongest degree down the axis of the mass, because of the circular form, and it would have the two sides of the point in the centre of the simple ring, which *seemed* to possess attractive and repulsive powers on the pole, removed to the ends of the cylinder; giving rise to two points, apparently distinct in their action, one being attractive, and the other repulsive, of the poles of a magnet. Now conceive that the pole is not confined to a motion about the sides of the ring, or the flat spiral, or cylinder, it is evident that if placed in the axis of any of them at a proper distance for action, it, being impelled by two or more powers in equal circles, would move in a right line in the intersection of those circles, and approach directly to, or recede from, the points before spoken of, giving the appearance of a direct attraction and repulsion: and if placed out of that axis, it would move towards or from the same spot in a curve line, its direction and force being determined by the curve lines representing the active forces from the portions of wire forming the ends of the cylinder, spiral, or ring, and the strength of those forces.

Thus the phenomena of a helix, or a solid cylinder of spiral silked wire, are reduced to the simple revolution of the magnetic pole round the connecting wire of the battery, and its resemblance to a magnet is so great, that the strongest presumption arises in the mind they both owe their powers, as M. Ampere has stated, to the same cause. Filings of iron sprinkled on paper held over this cylinder, arranged in curved lines passing from one end to the other, shewing the path the pole would follow, and so they do over a magnet; the ends attract and repel as do those of a magnet; and in almost every point do they agree. The following experiments will illustrate and confirm the truth of these remarks on the action of the ring, helix, or cylinder; and will shew in what their actions agree with, and differ (for there are differences) from, the action of a magnet.

A small magnet being nearly floated in water by cork, a ring of silked copper wire, fig. 16, having its ends connected with the battery, was brought near its poles in different positions; sometimes the pole was repelled from, sometimes attracted into, the ring, according to the position of the pole, and the connexions with the battery. If the wire happened to be opposite to the pole, the pole passed sideways, and outwards when it was repelled, and sideways and inwards when it was attracted; and on entering within the ring and passing through, it moved sideways in the opposite direction, endeavouring to go round the wire. The actions also presented by M. de la Rive's ring are actions of this kind, and indeed are those which best illustrate the relations between the ring and the pole; some of them have been mentioned, and if referred to, will be found to accord with the statement given.

With a flat spiral the magnetic power was very much increased; and when the rings were not continued to the centre, the power of the inner edge over the outer was well shewn either by the pole of a needle, or iron filings. With the latter, the appearance was extremely beautiful and instructive; when laid flat upon a heap of them, they arranged themselves in lines, passing through the ring parallel to its axis, and then folding up on either side as radii round to the edge, where they met; so that they represented, exactly, the lines which a pole would have described round the sides of the rings: and those filings which were in the axis of the rings, stood up in perpendicular filaments, half an inch long and so as to form an actual axis to the ring, tending neither one way nor the other, but according in their form and arrangement with what has been described; whilst the intermediate portion also formed long threads, bending this way and that from the centre, more or less, according as they were further from, or nearer to, it.

With a helix the phenomena were interesting, because according to the view given of the attractions and repulsions, that is of the motions toward and from the ends, some conclusions should follow, that if found to be true in fact, and to hold also with magnets, would go far to prove the identity of the two. Thus the end which seems to attract a certain pole on the outside, ought to repel it as it were on the inside, and that which seems to repel it on the outside, ought to appear to attract it on the inside; *i. e.*, that as the motions on the inside and outside are in different directions for the same pole, it would move in the one case to and in the other case from the same end of the helix. Some phenomena of this kind have been described in explaining figs. 8, 11, 12, and 13; others are as follows.

A helix of silked copper wire was made round a glass tube, the tube being about an inch in diameter; the helix was about three inches long. A magnetic needle nearly as long was floated with cork, so as to move about in water with the slightest impulse; the helix being connected with the apparatus and put into the water in which the needle lay, its ends appeared to attract and repel the poles of the needle according to the laws before-mentioned. But, if that end which attracted one of the poles of the needle was brought near that pole, it entered the glass tube, but did not stop just within side in the neighbourhood of this pole (as we may call it for the moment) of the helix, but passed up the tube, drawing the whole needle in, and went to the opposite pole of the helix, or the one which on the outside would have repelled it. On trying the other pole of the magnet with its corresponding end or pole of the helix the same effect took place; the needle-pole entered the tube and passed to the other end, taking the whole needle into the same position it was in before.

Thus each end of the helix seemed to attract and repel both poles of the needle: but this is only a natural consequence from the circulating motion before experimentally demonstrated, and each pole would have gone through the helix and round on the outside, but for the counteraction of the opposite pole.

It has been stated that the poles circulate in opposite directions round the wires, and they would consequently circulate in opposite directions through and round the helix; when, therefore, one end of the helix was near that pole, which would, according to the law stated, enter it and endeavour to go through, it would enter, and it would continue its course until the other pole, at first at a distance, would be brought within action of the helix; and, when they were both equally within the helix and consequently equally acted on, their tendency to go in different directions would counterbalance each other, and the needle would remain motionless. If it were possible to separate the two poles from each other, they would dart out of each end of the helix, being apparently repelled by those parts that before seemed to attract them, as is evident from the first and many other experiments.

By reversing the needle and placing it purposely in the helix in that position, the poles of the needle and the corresponding poles of the helix as they attract on the outside, are brought together on the inside, but both pairs now seem to repel; and, whichever end of the helix the needle happens to be nearest to, it will be thrown out at. This motion may be seen to exhibit in its passing state, attraction between similar poles, since the inner and active pole is drawn towards that end on the inside, by which it is thrown off on the outside *.

These experiments may be made with the single curve of M. de la Rive, in which case it is the wire that moves and not the magnet, but as the motions are reciprocal, they may be readily anticipated.

A plate of copper was bent nearly into a cylinder, and its edges made to dip into two portions of mercury; when placed in a current it acted exactly as a helix.

A solid cylinder of silked wire was made exactly in fashion like a helix, but that one length of the wire served as the axis, and the folds were repeated over and over again. This

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* The magnetizing power of the helix is so strong that if the experiment be made slowly the needle will have its magnetism changed, and the result will be fallacious.

as well as the former helix, had poles the same in every respect as to kind as the north and south poles of a magnet; they took up filings, they made the connecting wire revolve, they attracted and repelled in four parallel positions as is described of common magnets in the first pages of this paper, and filings sprinkled on paper over them, formed curves from one to the other as with magnets; these lines indicating the direction in which a north or south pole would move about them.

Now with respect to the accordance which is found between the appearances of a helix or cylinder when in the voltaic circuit, and a cylindrical common magnet, or even a regular square bar magnet; it is so great, as at first to leave little doubt, that whatever it is that causes the properties of the one, also causes the properties of the other, for the one may be substituted for the other in, I believe, every magnetical experiment: and, in the bar magnet, all the effects on a single pole or filings, *&c.*, agree with the notion of a circulation, which if the magnet were not solid would pass through its centre, and back on the outside.

The following, however, are differences between the appearances of a magnet and those of a helix or cylinder: one pole of a magnet attracts the opposite pole of a magnetic needle in all directions and positions; but when the helix is held along-side the needle nearly parallel to it, and with opposite poles together, so that attraction should take place, and then the helix be moved on so that the pole of the needle gradually comes nearer to the middle of the helix, repulsion generally takes place before the pole gets to the middle of the helix, and in a situation where with the magnet it would be attracted. This is probably occasioned by the want of continuity in the sides of the curves or elements of the helix, in consequence of which the unity of action which takes place in the rings into which a magnet may be considered to be divided is interfered with and disturbed.

Another difference is that the poles, or those spots to which the needle points when perpendicular to the end or sides of a magnet or helix, and where the motive power may be considered perhaps as most concentrated, is in the helix at the extremity of its axis, and not any distance in from the end; whilst in the most regular magnets it is almost always situate in the axis at some distance in from the end: a needle pointing perpendicularly towards the end of a magnet is in a line with its axis, but perpendicularly to the side it points to a spot some distance from the end, whilst in the helix, or cylinder, it still points to the end. This variation is, probably, to be attributed to the distribution of the exciting cause of magnetism in the magnet and helix. In the latter, it is necessarily uniform every where, inasmuch as the current of electricity is uniform. In the magnet it is probably more active in the middle than elsewhere: for as the north pole of a magnet brought near a south one increases its activity, and that the more as it is nearer, it is fair to infer that the similar parts which are actually united in the inner part of the bar, have the same power. Thus a piece of soft iron put to one end of a horse-shoe magnet, immediately moves the pole towards that end; but if it be then made to touch the other end also, the pole moves in the opposite direction, and is weakened; and it moves the farther, and is made weaker as the contact is more perfect. The presumption is, that if it were complete, the two poles of the magnet would be diffused over the whole of its mass, the instrument then exhibiting no attractive or repulsive powers. Hence it is not improbable that, caused by some induction, a greater accumulation of power taking place in the middle of the magnet than at the end, may cause the poles to be inwards, rather than at the extremities.

A third difference is, that the similar poles of magnets, though they repel at most distances, yet when brought very near together, attract each other. This power is not strong, but I do not believe it is occasioned by the superior strength of one pole over the other, since the most equal magnets exert it, and since the poles as to their magnetism remain the same, and are able to take up as much, if not more, iron filings when together, as when separated; whereas opposite poles, when in contact, do not take up so much. With similar helix poles, this attraction does not take place.

The attempts to make magnets resembling the helix and the flat spirals, have been very unsuccessful. A plate of steel was formed into a cylinder and then magnetized, one end was north all round, the other south; but the outside and the inside had the same properties, and no pole of a needle would have gone up the axis and down the sides, as with the helix, but would have stopped at the dissimilar pole of the needle. Hence it is certain, that the rings of which the cylinder may be supposed to be formed, are not in the same state as those of which the helix was composed. All attempts to magnetize a flat circular plate of steel, so as to have one pole in the centre of one side, and the other pole in the centre of the opposite side, for the purpose of imitating the flat spiral, fig. 15, failed; nothing but an irregular distribution of the magnetism could be obtained.

M. Ampere is, I believe, undecided with regard to the size of the currents of electricity that are assumed to exist in magnets, perpendicular to their axis. In one part of his memoirs they are said, I think, to be concentric; but this cannot be the case with those of the cylinder magnet, except two be supposed in opposite directions, the one on the inside, the other on the outside surface. In another part, I believe, the opinion is advanced that they may be exceedingly small; and it is, perhaps, possible to explain the case of the most irregular magnet by theoretically bending such small currents in the direction required.

In the previous attempt to explain some of the electro-magnetic motions, and to shew the relation between electro and other magnets, I have not intended to adopt any theory of the cause of magnetism, nor to oppose any. It appears very probable that in the regular bar magnet, the steel, or iron, is in the same state as the copper wire of the helix magnet; and, perhaps, as M. Ampere supports in his theory, by the same means, namely, currents of electricity; but still other proofs are wanting of the presence of a power like electricity than the magnetic effects only. With regard to the opposite sides of the connecting wire, and the powers emanating from them, I have merely spoken of them as two, to distinguish the one set of effects from the other. The high authority of Dr. Wollaston is attached to the opinion that a single electro-magnetic current passing round the axis of the wire in a direction determined by the position of the voltaic poles, is sufficient to explain all the phenomena.

M. Ampere, who has been engaged so actively in this branch of natural philosophy, drew from his theory, the conclusion that a circular wire forming part of the connexion between the poles of the battery, should be directed by the earth's magnetism, and stand in a plane perpendicular to the magnetic meridian and the dipping needle. This result was said to be actually obtained, but its accuracy has been questioned, both on theoretical and experimental grounds. As the magnet directs the wire when in form of a curve, and the curve a needle, I endeavoured to repeat the experiment, and succeeded in the following manner:—A voltaic combination of two plates was formed, which were connected by a copper wire, bent into a circular form; the plates were put into a small glass jar with dilute acid, and the jar floated on the surface of water: being then left to itself in a quiet atmosphere, the instrument so arranged itself that the curve was in a plane perpendicular to the magnetic meridian; when moved from this position, either one way or the other, it returned again; and on examining the side of the curve towards the north, it was found to be that, which, according to the law already stated, would be attracted by a south pole. A voltaic circle made in a silver capsule, and mounted with a curve, also produced the same effect; as did likewise, very readily, M. de la Rive's small ring apparatus*. When placed on acidulated water, the gas liberated from the plates prevented its taking up a steady position; but when put into a little floating cell, made out of the neck of a Florence flask, the whole readily took the position mentioned above, and even vibrated slowly about it.

As the straight connecting wire is directed by a magnet, there is every reason to believe that it will act in the same way with the earth, and take a direction perpendicular to the magnetic meridian. It also should act with the magnetic pole of the earth, as with the pole of a magnet, and endeavour to circulate round it. Theoretically, therefore, a horizontal wire perpendicular to the magnetic meridian, if connected first in one way with a voltaic battery, and then in the opposite way, should have its weight altered; for in the one case it would tend to pass in a circle downwards, and in the other upwards. This alteration should take place differently in different parts of the world. The effect is actually produced by the pole of a magnet, but I have not succeeded in obtaining it, employing only the polarity of the earth.

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* See Miscellanea.

Sept. 11, 1821.

ART. X. *Letter from* Dr. Hastings *to the* Editor, *respecting the Division of the Eighth pair of Nerves.*

SIR,

As the accuracy of my experiments on the par vagum, contained in a former Number of your *Journal*, has been established by those since conducted at the Royal Institution, I should not add one sentence to what has been already written on the subject, had not Mr. Broughton in his reply made personal allusions, which compel me to state,

That my observations were made in consequence of the accusation of inaccuracy, brought against me by that gentleman; and that I have again, with great care, compared them with his *first* paper, and can perceive no instance in which they are not fully applicable to it. In what part of Mr. Broughton's first paper does he say, that the contents of the stomachs of the rabbits operated on, were compared with those of the stomachs of healthy rabbits, similarly fed, and killed at the same time? where does he say that the eighth pair of nerves in the dog were found after death divided, or that he took care to place the animal in such a situation, that he must have observed it, if it had vomited any part of the milk? where does he say that he found the ends of the nerves uniformly apart from each other? what change does he mention in what he calls digested parsley, except change of colour? and does he not talk of the chyme of the rabbit resembling mucus, and being often found in greatest quantity in the cardiac portion of the stomach?

I am sorry that the discussion of this question, which it has been my wish to divest of all personality, should have produced any angry feelings in Mr. Broughton's mind; and remain,

Sir, Your obedient Servant,
CHARLES HASTINGS.

Worcester, July 18*th*, 1821.

ART. XI. *Meteorological Observations. By* J. F. DANIELL, Esq., F.R.S., *and* M.R.I.

I HAVE now completed the second year's observations with my hygrometer and the instruments connected with it, and propose, in this paper, to give a condensed abstract of their results. I have ceased to publish, quarterly, the Meteorological Journal at length, from a conviction that its utility, in such a state, is very doubtful, and that it would unprofitably occupy the room of more important or entertaining matter. Mr. Howard, in his late laborious work upon the climate of London, has shewn the proper use to be made of such accumulations of facts; it is only by collecting the means of different seasons, thereby neutralizing errors of observation and accidental irregularities, by arranging them in periods chosen according to the influence of particular circumstances, and by carefully collating and comparing them, however tedious the operation, that we can hope to arrive at certain consequences and useful results from the preliminary labour. Many, indeed, appear, at present, to be the observers of meteorological phenomena, to judge by the registers which regularly contribute their expletive powers to every magazine and journal which issue from the press; but, for want of proper direction and concert, their perseverance, it is to be feared, is wholly fruitless. The observers themselves rarely attempt to reason upon their observations, and no one will ever be found willing to sacrifice his time in arranging volumes of materials, when the very accuracy of the instruments with which they have been collected, may be doubted. But who can wonder at this want of co-operation, or to whom shall we look to turn this labour into a useful channel, when even the correctness of the tables published in the Transactions of the Royal Society, has been publicly called in question by Mr. Howard, Mr. Dalton, and Dr. Thomson? The parade of such observations, when confidence in their care has once been suspended, is worse than useless;—it is injurious to science.

Mr. Howard, in my humble judgment, has done more for the advancement of meteorology than all his predecessors or contemporaries, by his careful digest of observations contained in his admirable, and, I may add, entertaining, work. He has not only collected and arranged stores from which all future cultivators of the science must be content to draw, but has traced the path of useful application. At an humble distance I shall endeavour to tread in his footsteps; my only motive for holding the same course being a wish to establish the use of an instrument, which my own experience, as well as the acknowledgment of others, assure me, is entitled to be considered as a standard. Had Mr. Howard followed up the public approbation with which he has been pleased to honour my hygrometer by its adoption in practice, I should most willingly have surrendered into his abler hands the labour of observation; but as no one has yet undertaken to record its indications, I shall sedulously, if painfully, persevere, although the necessary division of my time is one, amongst other causes, which unfit me for the task.

With regard to the construction of the instrument, I have made one alteration which facilitates the observation. The condensation ball is now formed of black or other deep-coloured glass, and the dew is seen by reflected light, as upon a mirror. The most careless person may thus seize the exact moment of deposition with certainty; and, as no previous practice is necessary, the improvement may, probably, contribute to render it more popular.

The form in which I have extracted the observations from my register, the times of observing, and the division of the periods, are the first things that require remark. I have constructed three tables, which will be found to contain the greater part of the results necessary for our purpose. The first is similar to this one for the preceding year, published in the tenth volume of the *Journal*, p. 131. It contains the means of the different periods in large figures, and extremes in smaller, shewing the range of the several instruments in the respective intervals. The division of the year is into halves, quarters, and half-quarters, which I have found to be convenient and comprehensive. The grouping of the quarters represents Autumn, Winter, Spring and Summer; and agrees with that adopted by Mr. Howard, except that he commences each quarter with the second, instead of the first, week of the month. The reasons which he has assigned for departure from the customary division, are conclusive; mine, which was made before I had any knowledge of his work, approaches sufficiently near to excuse me from the laborious drudgery (not easily appreciated but by those who have undertaken such a task,) of recalculating the results. The commencing with Autumn was accidental. It was with this season that I began first to observe regularly, and of course at this season the first year ended. If this irregularity should be thought inconvenient, from the nature of the division, it may be easily rectified. The hours of the day at which the observations are made, are a consideration of greater moment; they are, nine in the morning, five in the afternoon, and eleven at night; very rarely, indeed, varying an hour from the appointed time. The maximum and minimum of temperature, by a register thermometer, may be added to these. If the selection of three periods in the day had been guided merely by meteorological considerations, I do not mean to say that these would have been the most proper; I think not: but it is not a question that is easily determined. They are not, however, destitute of peculiar advantages. The first object of repeated meteorological observations is to obtain accurate mean results. The most proper method of effecting this, even for the temperature, where we are assisted by an instrument which marks the changes in the absence of the observer, is still a subject of some dispute. The most we can hope for is a near approximation. Now the results of the barometer contained in the first column of Table 1, and those of the hygrometer in the second, third, and fourth columns, must be regarded first as the means of the particular hours at which the respective observations were made. It is further probable, that the former may be taken, without much fear of serious error, as the medium of the whole 24 hours. No particular known cause tends to prevent this assumption. But with regard to the latter, it is very different. The fall of the temperature during the night must exercise a very important influence upon the atmospheric vapour; and therefore the results of the second, third, and fourth columns, cannot be received as those of the 24 hours, without further inquiry.

The register thermometer has been the means of our attaining to a much greater degree of certainty with regard to temperature than we could ever have hoped to have arrived at by any other method. The same instrument will assist us much in our present inquiry. By referring to Tab. 3, (which is a table of temperature only, and which will be more fully explained hereafter,) it may be observed that the mean lowest temperature of the night is constantly several degrees below the mean constituent temperature of the vapour derived from the three before-named periods of the day. Now it is obvious, that once during the night the constituent temperature of the vapour cannot exceed that amount, nor is it at all probable that it should fall below it Speaking from experiment, observation, and calculation, I have no doubt that a precipitation of vapour takes place during some period of every night in the year. I do not now allude to the deposition of dew from the cold produced by radiation, (the amount of which for the whole year is likewise shewn by the table of temperature,) but to a precipitation in the body of the atmosphere itself. In the most cloudless nights of the whole year, when the stars are bright, and the disc of the moon perfectly sharp and well defined, by bringing the hygrometer out of a warm room, it will be found that the point of deposition is often three or four degrees above the existing temperature of the air; proving thereby that particles of water, though invisible, are floating in the atmosphere. The ball of the instrument, being hotter than the surrounding medium, forms an atmosphere of vapour, from these round itself, of greater density than the general one, and which, consequently, is precipitated at a higher degree. The notice of such an effect is perpetually recurring in my register. Thus then we are justified in adding another observation of the hygrometer, though indirectly obtained, to the three already recorded, by assuming the minimum temperature of the night as the minimum temperature of the vapour. The mean of these four observations form, I have no doubt, a very near approximation to the mean of the whole day. I have calculated these results for the last year, and have included the pressure of the vapour so corrected in the fifth column of Table 1. The constituent temperature of the vapour, corrected for both years, is included in the seventh column of Table 3. The difference is not so great as might at first be supposed.

The mean degrees of dryness require correction upon the same principle, but in a somewhat different manner. The same fourth observation, so obtained, is available for this purpose; but the register-thermometer furnishes us with another datum of importance to this correction, namely the maximum of heat. I have made this the basis of another observation, by assuming the medium of the constituent temperature of the vapour found by the fore and afternoon experiments, and taking the difference of that and the greatest temperature. The degrees of dryness, so corrected, are included in the sixth column of Table 1. It will be observed that they scarcely differ from the former; so that half the dryness of the day, at its maximum heat, may be considered as the average degree of the twenty-four hours.

The same remarks apply, in a great degree, to the amount of evaporation contained in the seventh column; but the correction in this case would be so exceedingly small that I have not thought it worth while to calculate it. The method of estimating the force of evaporation from the results of the hygrometer appears to me to be fully as correct as that in common use, and, if any method could be devised of measuring accurately the velocity of the wind, would be infinitely preferable. The capacity of the atmosphere for moisture at any given time is measured; the rapidity with which that capacity is saturated is dependant upon the temperature of the body which yields the moisture. These conditions vary almost *ad infinitum.* They vary on the land and on the water, they vary in sunshine and in the shade, they vary as land is more or less clothed with vegetation, or as water is more or less deep. The hygrometer is capable of following and appreciating all these changes, but the common gauge can only give the amount of evaporation from the shallow body of water in the place where it has been fixed. After all, the estimation of the mean evaporation from the surface of the earth is the most difficult problem of meterology. From the sea, and other deep bodies of water, whose temperature is tolerably equable, the process is pretty regular; but one part of a field exposed to the sun is often yielding moisture at the rate of eight or nine grains per minute from a circular surface of six inches diameter, while another part is only yielding one grain and a half.

The eighth and ninth columns contain the quantity of rain and the means of temperature, measured and calculated in the usual way. In Table 2, are inserted the mean results of both years in small figures, and the general mean in large. My great object has been to study facility of comparison, and I purpose, if I should continue my task, to collate in such a table the results of the current year, with the general average of all. This average will increase in accuracy as we proceed, and will, in a course of years, furnish a perfect standard by which to judge of the progress of the seasons. I am happy to find that the results which I have obtained with the barometer and thermometer correspond very closely with those deduced by Mr. Howard from a long series of years.

Table 3 is a general table of temperature, in which I have collected some particulars which were never before attended to, but which I am inclined to think may have much influence upon the general question. The first column contains the mean temperature, as usually estimated; the second and third, the mean highest and lowest; the fourth, the temperature at night, on the earth's surface, of such substances as are best fitted for radiation; the fifth the mean temperature of similar bodies exposed to the full influence of the sun's rays. This last is only complete for three quarters of a year, the idea having occurred to me last winter. My motives for adopting it I shall presently explain. The sixth column registers the mean constituent temperature of the vapour, and the seventh, the same corrected for a night observation as before explained. The extremes of all are included in small figures under their respective columns.

I shall divide the remarks I have to offer upon these particulars into two parts; the first shall include such observations as regard general consequences from the whole series, and the second shall consist of a short comparison of the several particulars of the two years.

The total weight of the atmosphere, and the grand problem of the variations of the barometer, are the first things that require our attention, and here it is worthy of remark, that no connexion can be traced between these fluctuations and the variations of the vapour. The barometer, in the last half quarter of the winter, when the pressure of the vapour is least, averages considerably higher than in the last half quarter of the summer, when the same pressure is greatest. A very cursory examination of the first and second columns of Table 2 will furnish plenty of instances of the like discrepancy. This result was certainly contrary to my expectations. Mr. Howard has inferred from his admirable system of averages, that "the winter barometer gains in its average .021 inches upon the autumnal; the vernal .030 inches or half as much more upon the winter; the summer .045 inches or half as much more still upon the vernal; but in the *autumnal* average, the whole difference is lost again, and the barometer comes back to its lowest level." In reasoning upon this gradation he ascribes the loss and gain to water; which is at one time converted into vapour, permanent as a part of the atmosphere for the season; at another dismissed in rain. In elucidating this connexion he continues, "Now in the *brumal* quarter, where we find the average of the barometer *lowest*, the temperature is lowest also; and there is every reason to conclude that the atmosphere in our district, and for many degrees of latitude and longitude around us, contains at this season, the lowest proportion of ponderable vapour." But this reasoning does not agree with the fact which he had just before stated; namely, that the average of the barometer is lowest in the *autumnal* quarter. Neither is the amount of this difference nearly sufficient to identify it with that of the vapour: it does not reach 0.1* inch while the excess of the summer vapour over the winter exceeds 0.2 inches. Moreover, if this influence were really appreciable in this manner, it is one of so constant a nature as must have shewn itself invariably in every recurrence of the seasons, not requiring to be elicited by a long series of averages.

But that there is a connexion between the great fluctuations of the atmosphere and the vapour which it contains at least in its precipitation, is evident from the following fact: of 674 observations of rain, hail, and snow, during the two years, only .94 took place, while the barometer was above the average of the season as deduced in Table 2. Of these 94 the greater part were very partial showers, chiefly in the winter quarters. Of the general results of the barometer alone, I shall not attempt to speak: the subject has been so ably elucidated by Mr. Howard, with such ample provision of observations, that it would be presumption in me to follow him, especially with such short experience. I am happy to observe that the mean of both years' observations agree with the mean as deduced by him for London: those periods where I differ, I consider as only oscillating round the true point, to which in a series of years they will be amenable.

The variations in the quantity and pressure of the aqueous vapour appear to be more equable, and to be regulated by more certain laws. In the winter the amount is least, and in the summer greatest, reaching very nearly to double. The spring and autumn quarters are intermediate, the latter having a slight excess over the former. There is, therefore, an evident connexion between these fluctuations and those of the mean temperature, which exactly follow the same gradation. Even the accidental small variations of the mean temperature, from the average, are uniformly accompanied by a corresponding alteration in the vapour. The utmost range appears to be from 0.090 to 0.631 inches, the mean pressure 0.327 inches, or rather, taking into the account the correction for the night, 0.313.

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* In Mr. Howard's work, Vol. II., page 281, this difference is misprinted 0.96 inches for .096.

As we have been unable to trace any general effect produced upon the motions of the barometer by the variations of the vapour, so are we unable to discover any influence of the former upon the latter.

But although the force of the vapour increases with the mean temperature, and reaches its maximum at the same period of the year, they do not travel together with equal steps. Mr. Howard deduces the difference of the heat of the seasons exactly equal; *viz.*, 11½ degrees for each quarter. There can be no doubt that this is correct. My own tables agree as nearly with the determination as can be expected, from so short a series. The difference comes out from them, spring above winter, 13°; summer above spring, 11°; autumn below summer, 12°; winter below autumn, 12°. But the gradation of the constitutent temperature of the vapour is as follows:—Spring above winter, 9°; summer above spring, 11°; autumn below summer, 8¼; winter below autumn, 11°. The circumstances of the case prove, I think, that this series approximates very nearly to the correct progression. If the difference had been equal, as in the mean temperature, the dryness also of the seasons would have been equal: but now, from winter to spring, the capacity of the air for moisture increasing faster than the supply, the degree of dryness must also increase; we accordingly find it in the table rising from 1¾° to 7°. From spring to summer the increase is nearly equal, consequently the degree of dryness remains the same, 7°. From summer to autumn the temperature of the air decreases

12°, and that of the vapour only about 9°. We accordingly find that the dryness falls to 4°, which again diminishes, though at a less rate, from autumn to winter. The exact correspondence of the consequences, and their precise march, we cannot expect to ascertain, but from the average of a series of years; but enough, I think, is apparent, to enable us to speculate a little upon their causes. The sea, and other deep bodies of water, are, no doubt, the grand reservoirs from whence is supplied the greater part of the vapour of the atmosphere; and the cooling and heating of such bodies are regulated by particular laws. It is a well known fact, that the heat of water cannot be raised above 40°, till every particle of the mass, however deep, has attained that temperature; but that beyond this, the superficial water may be heated, without affecting the lower stratum. So in cooling, the whole volume must be cooled down to 40° before the superficial water can fall below that point. Now the Spring quarter, from March to May, includes, probably, the period when the whole body of water, in this climate of the globe, rises to the point of 40°. A large portion of heat must, therefore, be expended, in warming its entire bulk; and, consequently, its increase cannot keep pace with the surrounding air. In the next quarter, however, having attained its maximum of density, the whole heat is expended upon the superficial water alone; and the increase of heat in the upper stratum of water, and consequently, in the vapour, is commensurate with that of the air. The autumn again includes the point at which the whole mass of heated water must fall to 40°; which point being passed, the cooling of the upper stratum keeps pace with the cooling of the air. Daily observations upon the superficial temperature of the sea would greatly tend to elucidate this subject.

The rate of evaporation is in compound proportion to the heat of the water yielding the vapour, the degree of dryness, and the velocity of the wind. In the calculation taken in the tables, the temperature of the air has been always taken as the temperature of the evaporating fluid; whereas it is obvious that this can seldom, if ever, be the true state of the case. Did we but know the temperature of the sea at different hours of the day, and at different periods of the year, we should obtain, by the substitution, a much more useful result. The evaporation from the land must be much more irregular than that from the ocean and other deep accumulations of water: it must vary with almost every moment of the day, and every variety of situation. The fallow-field, and the field of corn, yield their moisture with different facility; and sun-shine and shade must produce still greater modifications.

I have found, from experiment, after a shower of rain, while the temperature of the air has been 66, and that of the vapour 60, that the temperature of a running stream has been 62; of wet grass, when the sun had been shining upon it, 69; of wet sand, 76; of stagnant water, 67; of damp garden-mould, in the sun, 84. So that at the very same moment of time these situations were respectively yielding at the rate of 0.18 grains, 0.89 grains, 1.82 grains, 0.67 grains, and 3.16 grains, per minute, from a surface of 6 inches diameter. Not that vapour of these various degrees of force existed at one time; for it is evident that those which were above the temperature of the air must have been instantly precipitated, and as instantly redissolved at the general temperature.

The facility and rapidity with which vapour of such different degrees of force in its nascent state is resolved into one general mass of equal density, is very worthy of observation. I have often endeavoured to detect it before its change, with the hygrometer, but with equivocal success. For example, when the temperature of the air was 56, after sun-set, and the hygrometer denoted the dew-point to be 47, I placed myself in the middle of a stream whose temperature was 63; the precipitation took place at 50, only 3° higher than before. As the water was here yielding vapour of 16° higher temperature than the general average, I expected to have found it more nearly approaching the point of saturation; especially as there was very little air stirring. From hence we must conclude that it diffuses itself with immense velocity, which, perhaps, we shall have the less difficulty in conceiving, when we call to mind that the rising vapour possessed a force of 0.578 inch, which was only opposed by one of 0.339 inch. When the general atmosphere approaches more nearly to the point of humidity, this rise of hotter vapour from water is very visible in the cloud which is often observed to hang over the course of rivers in a calm evening. In certain situations, also, where the free rise of vapour is opposed by impending obstacles, a small increase of elasticity may be observed. Thus, after a succession of wet days, when the weather had again become clear and settled, I have observed the temperature in the open country to be 64°, and the point of deposition 53°, when in the heart of a large wood they were respectively 62 and 55; the foliage of the trees forming a mechanical obstruction to equal diffusion.

It would be easy to reduce the data contained in the tables to the number of inches evaporated from a given surface, but in the present state of our knowledge it would be useless, or, what is worse, might tend to mislead. It is amusing to observe the attempts that are made to make the results of the common evaporating-gauge tally with those of the rain-gauge, when the former, so far from representing the circumstances of those bodies which yield the great body of vapour on the earth's surface probably does not correspond in all essential particulars with a dozen-puddles in the course of the year. The results of the hygrometer accommodate themselves more easily to the ever-varying circumstances, but even from these we can only at present infer the capacity of the atmosphere for moisture, modified by the velocity of the winds.

The quantity of rain which falls in different seasons is probably the most variable result that meteorology affords; but nevertheless some general principles may be derived from the observation. The greatest quantity falls in the driest half of the year; and this quantity, when no other circumstance modifies the conditions, appears to be in direct proportion to the quantity of vapour in the air. Thus, in the two half years from September to February, and from February to August, the average of the barometer is exactly similar, but the quantity of vapour in the latter exceeds that in the former by one-third: the quantity of rain is also as 3 to 2. At the same time, the degree of dryness is more than double, and the force of evaporation more than treble. It is also perfectly evident that the height of the barometer has a very material influence, as well as the temperature. To illustrate the former, without the latter, let us take the quarter of a year from March to May 1820, and compare it with the same quarter of 1821. The force of vapour was nearly alike, and the temperature of the latter rather higher than that of the former; nevertheless the rain of this quarter was more than double; the height of the barometer, in the first, being rather above the mean, while in the latter it was 0.14 inch below it. The power of temperature in modifying the precipitation is strikingly illustrated in the two half-quarters of October and November. In 1821 the barometer was lower, and the force of the vapour higher, than in 1820. Nevertheless, the quantity of rain in the latter was double that in the former, for the average mean temperature was 5¼° lower. The joint influence of the two will not fail to strike any one who takes the trouble to inspect the tables.

We now come to the consideration of temperature; and I have endeavoured to give a complete view of the subject in the third table. I have therein included three series of observations not usually taken into account. The first is the greatest effect of radiation at night, the second the highest temperature of the sun in the course of the day, and the third the constituent mean temperature of the vapour.

I can add nothing to the ingenious views of Mr. Howard with regard to mean and extreme temperature in general. My motive for including observations upon radiant heat both from and to the earth were, first, their probable importance to vegetation and agriculture, and, secondly, their obvious connexion both with the production and precipitation of vapour. The medium effect of radiation from the earth at night, it will be seen, is about 4°: the maximum which I have observed is 13°. It will probably strike many people with surprise, to find that there is but one period of six weeks in the year in which vegetation, in particular situations, is not exposed to a freezing temperature, and only one quarter of a year in which the thermometer so situated is not subject to fall as much as six or seven degrees below the point of congelation. The effect of situation is extremely curious in modifying this process. In the latter end of the month of September 1821, in a garden placed in a sheltered situation in a valley surrounded by low hills, I observed that all the vines and Dahlias were frost-bitten and turned quite black, while in another garden not 100 yards distant, upon the gentle acclivity of a hill, no signs of frost were to be seen. At another time, early in the autumn, in the same situation, the grass has been quite rigid with hoar frost, while upon the hill nothing but a light dew was to be perceived. The effect of this cold upon vapour is sometimes very visible. In sheltered situations, and in very calm weather, a low mist may often be seen rising in the meadows, when the surrounding spaces are quite clear. If the ground which this mist covers be examined, it will constantly be found to be 5° or 6° below the heat of the clear regions. The slightest breeze is sufficient to mix the air of the two, and to dissipate the cloud. This will often even be effected by a person merely walking through it.

With regard to the quantity of dew deposited upon filamentous substances, I have found that a piece of cotton exposing a flat circular surface of two inches diameter, gained in weight twenty grains, when the force of the vapour was .481 inch, and the radiating thermometer 8° below the lowest standard, that is to say when the two thermometers were respectively 58 and 50. At another time, when they were only 45 and 37, the same piece of cotton had only gained eleven grains, the force of the vapour being .316, and the difference of temperature the same. It is most probable, that in neither instance had there been any loss from evaporation, as at the time of examination the hygrometer denoted the complete saturation of the atmosphere. Calculating from these experiments, and the mean force of vapour and radiation, the average deposition of dew for every night in the year, upon a like surface, would be about six grains.

The results of the experiments upon the direct radiation of the sun are still very incomplete; but I cannot help hoping, that when sufficiently advanced, they will afford matter for useful speculation. What is the difference of the effect in different years? What are its effects in maturing the fruits of the earth? What is its connexion with the production of vapour? How is its energy spent when veiled from the earth by clouds? Its average for the half year, from March to August, was 27° above the mean highest in the shade, its maximum effect 144°. Many reflections readily suggest themselves upon this subject, but it would be premature to indulge in them at present.

The mean constituent temperature of the vapour stands in an interesting relation to the other particulars of general temperature. Its connexion with the average of the mean is very obvious, and it appears to be influenced, in some measure, by the mean lowest, which it never exceeds by more than 4½°. The average degree of dryness for the whole twenty-four hours, by this comparison, would appear to be 1° below that obtained by the three direct experiments, and the correction before applied. But it must be remembered, that although the force of the vapour is affected by the maximum cold of the night, the degree of dryness is not liable to its influence. After the point of humidity is once attained, which it generally is long before the minimum, a further reduction of temperature does not alter the circumstances; so that an observation of the lowest temperature of the night as included in the general mean temperature, is not applicable to this purpose.

I have now, in conclusion, a few remarks to make upon the comparison of the two years. They will not be found to differ much in their general averages, but in their particulars very materially. The year 1819—1820 was, however, drier than the year 1820—1821, and a greater depth of rain fell in the latter than in the former. The first was also distinguished very much more by extremes than the second, all the instruments except the barometer denoting a very much wider range.

The autumns differed very essentially in their characters. In the first half-quarter the year 1819 was 1¾° drier, and 2¼°

warmer, than 1820. The depth of rain was, however, greater; for the barometer was not so high, and the vapour was more dense. As this is the season of the year when the most important fruits of the earth come to maturity, and the securing of them in good order is the most anxious care of the human race in these latitudes, the state of the weather at this time acquires a proportionate interest. In 1819 it was remarkable for being dry, clear, and warm. The finest harvest that had been known for years was housed in the most satisfactory manner. Not only in this country, but throughout Europe, it formed almost an epoch, and corn-fields, orchards, and vineyards, shared in the general benefit. The mean results of this period may therefore be considered as the standard of a fine season. The turnip-fields indeed, so important a part of English agriculture, suffered from great drought and never recovered, but the grasses, and pasture in general, though burnt in the beginning of the autumn, revived with the rains in the last half-quarter.

In the year 1820 the harvest was much later. The crops of corn, though abundant, were not of so fine quality as the last, and were much mildewed; but upon the whole, this was also reckoned a productive harvest. The weather was still too dry for the turnip-crops.

The second half-quarters of the autumn were precisely similar in point of dryness, but in 1820 the temperature exceeded that in 1819 as much as it fell below it in the first six weeks, making the averages of the whole quarters precisely the same. This accession of heat probably prevented the precipitation of the usual quantity of water, for the amount of rain was less than half.

The winters differed still more widely than the autumns. The first was remarkable for its severity, and the second for its mildness: the respective mean temperatures being 33 and 38. In this quarter the latter regained the dryness which it was behind in the preceding; and the means of the two half-years were exactly similar. This state of the atmosphere is reckoned by no means unfavourable to the farmer, and neither in the cold season of 1819-1820, or in the dry season of 1820-1821, were any complaints made. The last winter half-quarter of 1821 was particularly remarkable for a very high average of the barometer.

In the first half-quarter of the spring the year 1820 was very backward; the wheats looked very indifferent, and vegetation altogether very unpromising. The blossoms of fruit-trees were very much injured by frosts and cold winds. In the corresponding period of 1821, the weather, on the contrary, was extraordinarily fine and open. The operations of husbandry were unusually forward; the wheat was vigorous and firmly rooted, and every appearance of vegetation as flattering as could be wished. The former period was distinguished from the latter, by being 1° drier, and $2\frac{1}{2}$° colder, a much higher barometer, and half the quantity of rain.

In the second half-quarter, the advantage began to turn in favour of the first year. The temperature was higher, and the dryness continued. There was a sufficiency of rain, in the form of warm showers; the appearance of the wheat improved, and barley and oats promised very well. In the second year vegetation was checked by cold north winds: pasture was not forward, but still the whole prospect was good. At the commencement of the summer of 1820, the weather turned extraordinarily hot; the change was very sudden, and the produce of the fields made astonishing progress to maturity. The harvest commenced early, and, although the weather was rather unsettled, was well secured. The produce of all kinds was abundant, though not of the first quality.

The summer of 1821 was extremely backward, but favourable for growing wheats. The lowness of temperature was considered, at the time, rather favourable, as tending to check over-luxuriance of vegetation: barley, however, suffered materially from this cause. Near the usual harvest-time, the corn, though full-eared, had hardly completed the flowering process. Oats were heavy, full-eared, and promising. Turnips, and all kinds of pasture, particularly fine and luxuriant. A succession of hot days, at the latter part of the summer-quarter, raised the average temperature above the corresponding period of the first year, and rapidly brought on the ripening of the grain. Nothing was now wanting, but a favourable dry period to house the harvest. The reaping did not begin till the 25th or 26th August, more than a fortnight later than the usual time.

This summer must be reckoned altogether wet and cold, and owing to this, it is feared that the vintage on the Rhine, the Elbe, and in Switzerland, will entirely fail.

The great characteristic features of the two years were, in the first, a cold winter and a hot summer; and in the second, a very mild winter, and a backward cold summer.

ERRATA IN TABLE, Vol. X, p. 131.

Column of Temperature, February, *for* 31 *read* 32½
— 29 — 31
August, — 63 — 61

ART. XII. *An Account of some Observations and Experiments made by* MR. GREEN, *during his ascent in a Balloon from Portsea, on the 6th of September*, 1821.

[Communicated by R. H. Solly, Esq., F.R.S. and M.R.I.]

Mr. Green has the merit of being the first person who has tried experiments upon the buoyant powers of coal-gas. In some of his preliminary trials, he ascertained that the ascensive force of a small balloon three feet in diameter, when filled with this air, was equal to eleven ounces, and when filled in the common way with hydrogen not more than fifteen ounces. He has since made three voyages in a balloon filled from the street-pipes, thereby reducing the expense of such ascents to a mere comparative trifle.

During his last ascent he made some experiments of much interest to science, and which, as he has promised to repeat them at no distant period, may, probably, furnish data of the utmost importance. The operation of filling the balloon commenced about eleven o'clock on Thursday the 6th of September, at Portsea, under the superintendence of the engineer of the gas company; the operation was completed by half-past twelve. The morning, although cloudy, seemed nevertheless to presage favourably. At half-past one, Mr. Green took his seat in the car, and immediately began to ascend slowly and majestically. On leaving the ground the barometer stood at 29.7 and the thermometer at 74°. The balloon took a N.E. direction, and he found that he could ascend to a great height without entering the clouds. After he had attained a height of about 4,500 feet, he launched a parachute with a cat which fell in safety; its descent was very rapid for about 300 feet, it afterwards became distended, and its oscillation was very inconsiderable: it reached the earth in about eight or nine minutes after its separation from the balloon. He now ascended with great rapidity to upwards of 10,000 feet. After completing his observations which are annexed, Mr. Green at twenty minutes after two determined to descend. By repeatedly discharging ballast the descent was rendered gradual, till half-past two; at this time a heavy shower of rain added considerably to the weight of the balloon, and occasioned likewise the condensation of the gas, accelerating the fall in a manner that might have been dangerous, had it not been checked by throwing out a large quantity of ballast. He at last landed in safety in a meadow near Frimley, Surrey, having completed a voyage of forty-five miles in forty-five minutes, in a direction N.N.E. from Portsmouth.

During this voyage, the attention of Mr. Green was chiefly directed to the indications of the barometer, thermometer, and Daniell's hygrometer. His observations on these instruments are included in the following table.

TIME.			Barom.	Therm.	Dew Point.
15′	before	2	29.7	74	
11	—	2	26.8	70	
7	—	2	23.2	72	
4	—	2	21.4	69	64
10	after	2	20.5	45	32
12	—	2	20.3	38	25
26	—	2	21.9	40	37
28	—	2	26.1	48	rain

At the point of greatest altitude Mr. Green filled two bottles, which had been previously prepared and filled with distilled water, with the atmospheric air; these have been since examined at the laboratory of the Royal Institution by Mr. R. H. Solly and Mr. Faraday; the first was weighed and opened under distilled water of the temperature of 60°, 660.5 grains of water entered, the whole quantity which the bottle would hold being 1,910 grains. The second bottle, when full, held 1,916 grains of water; the quantity which entered when opened was 636.7 grains.

The average of these two experiments gives rather more than one-third for the diminution of density of the atmosphere at the height to which Mr. Green ascended. This agrees very nearly with the results of the barometer. The analysis of the air confirmed the observation of M. Gay Lussac, that no difference can be detected in the atmospheric air taken from great altitudes and at the surface of the earth: the experiments were made by explosion with hydrogen, and the comparison of the condensation made over mercury between the air of the bottles and that of the laboratory; the differences were perfectly immaterial, sometimes on one side and sometimes on the other, evidently arising from unavoidable errors in experimenting.

Remarks by Mr. Daniell.

It is much to be regretted that Mr. Green omitted to take the point of deposition before he commenced his ascent, or after he had again landed, as such an observation would have added greatly to the value of his experiments; as they stand, they will, however, form a valuable addition to any future observations which it is to be hoped that he may be induced to make. Some interesting particulars may even now be derived from them. I have subjoined a calculation of the different heights as observed by the barometer, and the density of the vapour at those heights, as indicated by the hygrometer. The different degrees of dryness are also added in the following table.

TIME.	Barom.	Therm.	Dew Point.	Force of Vapour.	Height. Feet.	Dryness.
15 before 2	29.7	74				
11 — 2	26.8	70			2952	
7 — 2	23.2	72			7288	
4 — 2	21.4	69	64	.597	9893	5°
10 after 2	20.5	45	32	.200	11059	13°
12 — 2	20.3	38	25	.156	11293	13°
26 — 2	21.9	40	37	.237	8813	3°
28 — 2	26.1	48	48	.351	3630	0°

The state of the atmosphere on the day of Mr. Green's ascent was evidently not favourable for ascertaining the grand question of the rate of decrease in the density of the aqueous vapour, when the atmosphere is undisturbed. By an experiment which I made myself in the country not very distant from the point at which he descended, the degree of precipitation on the earth's surface, at two o'clock, was 64°, exactly the same as he found it at the height of 9,893 feet; so that it is very probable that there was but little difference in the density of the vapour in that immense stratum. The very small decrease in the temperature agrees with this supposition, which also accounts for the great height to which the balloon ascended without entering the clouds ; thus the dryness would have decreased from 10°, on the surface of the earth to 5°, and before the next observation I should conjecture that the aëronaut must have passed the point of precipitation through a bed of clouds. After crossing this plane the temperature and the force of the vapour rapidly decreased, and the degree of dryness increased with the ascent.

The condition of the atmosphere appears to have varied considerably with the progress in-land, and in receding from the sea the force of the vapour seems to have decreased, and the temperature of the air still more rapidly. The point of deposition in the descent was passed in a shower of rain, at a distance of not more than 3,630 feet above the surface of the earth, the constituent temperature of the vapour having fallen 16°, and the temperature of the air not less than 24° in the short space of thirty-five minutes. The increase of the density of the vapour towards the surface of the earth, was much more gradual during the descent over the land than during the ascent over the sea: was the difference owing to a large body of vapour rising from the sea? The temperature of the water at the time would have been a valuable addition to the observations.

It is to be hoped that Mr. Green will be enabled to persevere in his exertions, as a continuation of such observations cannot fail to be of the utmost use to science. It would be desirable also that he should be a little more particular in noticing the different indications of the atmosphere, that is to say, the course of the wind on the surface of the earth, and the direction of the different currents—the height of the clouds and the temperature when crossing them, and whether there is more than one stratum at different heights. Now that he is aware of the tendency of the different experiments, there are, no doubt, many particulars which will readily suggest themselves to him, and he will not, of course, neglect to make observations before his departure, and after his return, if circumstances will permit.

ART. XIII. *Proceedings of the Royal Society.*

The following papers have been read at the table of the Royal Society, since our last report.

May 31. Some experiments on temperature, with a view of determining the ratio of temperature, and the point of absolute cold, by JOHN HERAPATH, Esq., communicated by DAVIES GILBERT, Esq., F.R.S.

June 7. An account of the re-measurement of the cube, cylinder and sphere, used by the late Sir GEORGE SHUCKBURGH EVELYN, in his inquiries respecting a standard of weights and measures: by Capt. HENRY KATER.

21. An account of observations made at the observatory of Trinity College, Dublin, since 1818, for investigating the parallax and aberration of the fixed stars, and the effects of lunar nutation: by the Rev. JOHN. BRINKLEY, D. D.

28. On the effects produced in the rates of chronometers by the proximity of masses of iron: by PETER BARLOW, Esq., communicated by JOHN BARROW, Esq.

July 5. Some positions respecting the influence of the voltaic battery, in obviating the effects of the division of the 8th pair of nerves, by A. P. WILSON PHILIP, M. D., communicated by B. C. BRODIE, Esq.

On the magnetic phænomena produced by electricity, and their relation to heat, occasioned by the same agent: by Sir HUMPHRY DAVY, Bart., P. R. S.

12. An investigation of some theorems relating to the theory of the earth, and the principle of equilibrium in fluids, by M. HOENE WRONSKI, communicated by JOHN POND, Esq., Astronomer Royal.

On the peculiarities that distinguish the Manatee or Dugong of the West from that of the East Indies, by SIR EVERARD HOME, BART., V. P. R. S.

On a new compound of Chlorine and Carbon, by Messrs. R. PHILLIPS and FARADAY, communicated by the President.

On the Nerves; giving an account of some experiments on their structure and functions, which lead to a new arrangement of the system, by CHARLES BELL, ESQ., communicated by the President.

ART. XIV. ANALYSIS OF SCIENTIFIC BOOKS.

Philosophical Transactions of the Royal Society of London, for the Year 1821. *Part I.*

IN the limits assigned to this portion of our Journal, it will not be possible to enter into a detailed account of the important papers contained in the present half-volume of the Philosophical Transactions; we shall therefore aim at presenting our readers with a succinct view of the principal novelties which they contain, and with the heads of the leading subjects which they discuss.

The chemical papers, once forming so prominent and important a feature in the publications of the Royal Society, have lately suffered a serious defalcation, both in number and interest: in the part now before us there is, however, a very interesting essay on the compounds of chlorine and carbon, by Mr. Faraday, the chemical assistant in the laboratory of the Royal Institution: there is also a communication from Dr. Henry on the aëriform compounds of charcoal and hydrogen, which may be considered as supplementary to a memoir on the same class of bodies inserted in the Transactions for 1808.

Physiology was so favourite a study with the late president, and his means of patronising and propagating the pursuit were so extensive, and generally so well directed, that the Transactions, during the long series of years which he filled the chair of the Society, were the depositary of the principal investigations and discoveries in that department of knowledge; and comparative anatomy, which may be regarded as its basis, was proportionally promoted under the same auspices; it is, however, remarkable, when the botanical propensities of the late Sir Joseph Banks are taken into the account, that the structure and physiology of vegetables have been very sparingly and imperfectly noticed in the publications of the scientific body over which he presided; whereas the anatomy and functions of the animal creation are prominently treated of, and sumptuously illustrated, in almost every volume of the Transactions published during the last five-and-twenty years. For these contributions we are principally indebted to the indefatigable exertions of Sir Everard Home, who has thus shown that the abstract pursuits of science are not incompatible with laborious and extensive surgical avocations, and who, in furnishing the student with an example of the true means of acquiring professional celebrity, has also opened to himself an inexhaustible mine of occupation and amusement during those hours of leisure and retirement which declining years necessarily bring with them. This part of the Transactions contains two communications from Sir Everard upon physiological subjects; under the same head we may class an account of the urinary organs of two species of frogs, by Dr. Davy; and two letters addressed to the President, showing a singular influence of the male upon the progeny of the female.

Geology has not of late years constituted any prominent feature in the Transactions of this Society; this is partly to be attributed to the speculative tone of much that has been written upon this subject, and which renders it unfit for the Transactions of a society for the *promotion* of natural knowledge; and partly to the magnificent publications of the Geological Society; in which, with much that is light and trifling, there are many papers of standard excellence and laborious research. How far this and other similar philosophical associations affect the general interests of British science, is a question of such pith and moment, as to require extended and serious consideration. In our opinion, (which has certainly not been hastily, and we trust not superficially formed) with much good, they have been productive of more harm; they have in some instances perhaps brought talents into the field which might otherwise have lain dormant; they have also excited a temporary interest in the welfare and pursuit of science among a class of persons whose names are only known as contributors to the funds of the establishments; but then, they have created petty dissensions and paltry jealousies among those who once were friends and colleagues, and have tended to scatter and subdivide the forces of science in a way most unpropitious to its true welfare. We remember when all that was eminent in philosophy and excellent in art was to be found at the weekly meeting of the Royal Society in Somerset-Place; he who wanted information sought and found it there; strangers assembled to converse with the learned of the land; and, though a hostile breeze now and then ruffled the good understanding and friendly intercourse of the members, it soon subsided into a prosperous calm. But now, the forces of science are not merely scattered but disunited, and among philosophers as among politicians, we have, under other names, tories, whigs, and radicals. He who wants to consult the learned and examine the records of science, is driven from the east to the west, and from the north to the south; up stairs, down stairs, and even sometimes into my lady's chamber, before he can find the object of his search and inquiry; losing that time which might be occupied in digesting and extending his information, in traversing the most opposite quarters of the metropolis, mounting into back-rooms up two pair, calling in Moor-fields for what is only to be had in Albemarle-street, or the agreeable reverse; and at length finding that, for want of method and good understanding, he loses a week in London in search of that which a day would effect in Paris. We, however, by no means intend to hold up the literary and scientific establishments of France as deserving unalloyed praise or unqualified imitation; there is about them, that jobbing servility, and cringing subserviency, which betrays their shackles and dependance.

But, not to lose time in the further enumeration of the serious and multiplied inconveniences which obviously result from the divisions and subdivisions into which the scientific establishments of the metropolis are split, let us, briefly as may be, inquire whether any tangible remedy is at hand, which, while it includes the advantages of the disunion, may at the same time heal the dissensions which it creates, and fill up the chasms which it has induced.

Should government be seriously memorialized and appealed to in behalf of the semi-bankrupt state of our scientific bodies in general? should we humbly pray relief, and suggest the benefit that would accrue from the establishment of honorary rewards, and other distinctions, which very moderate pecuniary aid would confer? Should they be told that the apartments of the Royal Society in Somerset-House are altogether inadequate; that they are out of repair; that the valuable library is scattered through various rooms for want of one large enough to receive it; that the Society has been obliged to dispose of a variety of instruments and apparatus in consequence of the expense of hiring an apartment for their preservation? should not the Society at once pray for a palace in the new street, appropriately convenient and magnificent, and humbly request that the sum of 50,000*l.* be further granted to the body to defray various necessary expenses, and to establish a fund for its future exigencies? Should we not hint at the *Jardin des Plantes*, and its sumptuous museum and amphitheatre; at the palace of the Institute, and the magnificent hall of the Louvre appropriated to its members. To this and much more, the Chancellor of the Exchequer might reply nearly as follows: "Gentlemen; His Majesty's government has at all times shewn its disposition to promote the objects of science when fit opportunity has presented itself, but under existing circumstances, I should not feel justified in advising the increased expenditure which the adoption of your request would involve: the Parisian establishments, to which you have alluded, have not conferred such exaltation and improvement upon science as to render them objects of our imitation, for with means derived from private sources, you have achieved conquests in the dominions of knowledge which are without equal; and as neither the late Mr. Cavendish, nor Sir Joseph

Banks, who, as president of the Royal Society during a very extended period, must have been well acquainted with the real interests of science, thought fit to bequeath a small fraction of their enormous property, to the establishment of scientific prizes, or to any other similar purpose, it would be presumption on the part of His Majesty's government to adopt a measure, which, however plausible in appearance, must be open to objections and liable to misapplication." We do not mean to consent to this reply, because we are convinced that even in the present state of things, a very few thousand pounds, granted with discernment to the calls of science, would produce effects incalculably beneficial; we are also convinced that the Rumford and Copley medals, which are so judiciously bestowed by the council of the Royal Society [*], have been productive of much honourable emulation; but yet we think the opposite argument fairly inferable from the premises.

If, however, we are not in error, the remedy of the evils adverted to is not necessarily dependant upon extraneous aid. That the Royal Society, as the parent establishment, claims precedence, will not be disputed; let it therefore stand at the head of a Scientific College, and let the other Societies arrange themselves as committees, each as heretofore pursuing their individual avocations, under a chairman, a president, and officers of their own; let the meetings of the Royal Society, and of the various scientific committees, be held under the same roof, and their publications appear in the same volume, either as communications to the Royal Society, or as the Transactions of the various committees; let the books, instruments, and collections also be preserved in the same building; and let the expenses of the whole be defrayed from a general fund raised as heretofore, by subscriptions to the Society and its committees, but rendered infinitely more effective by concentration upon one object, instead of being frittered away in the small *items* of separate establishments.

We are quite aware that this scheme will be called Utopian and chimerical, but much that has been thought impossible has come to pass; the steam-engine has done wonders in this way; and when gas-lights were first talked of, we remember one of the most eminent and profound men of science in this or any other country, asserted that " a company might as well be formed for lighting London with a slice of the moon, as for carrying the gas from pit-coal through the streets of the metropolis." It is not therefore impossible, we even trust, not improbable, that if there were that desirable union among scientific establishments to which we have adverted, the nation might at least afford such assistance as would enable them to be suitably, and even sumptuously, lodged. We have indeed heard that it is in contemplation to remove Carlton Palace, to continue the new street down to Westminster Abbey, and to erect in an open part of it, opposite to the Horseguards and Whitehall, a building appropriated to the Royal Society and the Royal Academy: if this be so, we trust that the British Museum will not be forgotten; and that its treasures will be removed to some repository less frail than that which now contains them; not one of those evanescent combinations of lath, plaster, cement and brickbats, which adorn Waterloo-Place, but a good honest stone building, which, in our opinion, could not be better situated than upon the site of the present edifice. It has, indeed, been rumoured, that the Duke of Bedford has thrown difficulties in the way of such an undertaking; but surely the ungracious impediment of which we heard could not come directly from a person possessed of less patriotism than his Grace, for the houses which surround the garden of the present Museum would be rather improved than injured by the erection of a series of courts and galleries for the reception of the various collections; there is "rottenness" somewhere, or something would, long ere this, have been accomplished towards redressing those grievances of which we have only given a scanty outline, and of which not only the scientific world, but the public at large, have a right to complain.

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[*] We wish it had been permitted to us to quote Dr. Wollaston's admirable discourse to the Royal Society, upon the 30th of November last, when the Copley medal was adjudged to Professor Oersted of Copenhagen. It spoke volumes to the point before us.

The casual mention of the Geological Society led us to this digression, which is not, we hope, altogether misplaced in an examination of the labours of the Royal Society, to which we now revert. One geological paper is contained in this part of the transactions, relating to a subject which requires more attention than it has received; and if the author be not in error, he has discovered a fact of much curiosity, and some importance. The mountain limestone of Plymouth lies directly upon clay-slate, and is remarkably scanty in organic remains; it includes certain caverns, perfectly insulated, some of which are incrusted with stalactite, and present nothing remarkable. In others, Mr. Whidbey, the author of this communication, has discovered certain fossil bones; these caves were without any stalactitical incrustation, having only a little dry clay at the bottom, and we beg the reader's attention to the following particulars: "The cavity was *entirely* surrounded by compact limestone rock, about eight feet above high-water mark, fifty-five feet below the surface of the rock, one hundred and seventy-four yards from the original face of the quarries, and about one hundred and twenty yards in that direction from the spot where the former bones were found in 1816." The bones, as appears from Sir E. Home's annexed catalogue, are those of the rhinoceros, the bear, and an animal of the deer kind, and of another animal of the size of the bear. Mr. Whidbey abstains from any speculations respecting the catastrophe by which these bones were thus entombed in the solid rock, and properly enough rests content with a naked statement of what he considers to be the fact.

The late volumes of the *Philosophical Transactions* have abounded in important contributions to physical science, and the part before us is, in this respect, not deficient; it includes a paper on the magnetic phænomena produced by electricity, by Sir H. Davy; two important communications, results of the accurate diligence of Captain Kater; and a notice from the same, respecting a volcanic appearance in the moon; and lastly, an account of a micrometer made of rock crystal, by Mr. Dollond.

Having thus generally surveyed the contents of this volume, we shall proceed in a few instances to particularize their details.

i. *On the black Rete Mucosum of the Negro, being a Defence against the scorching Effects of the Sun's Rays.* By Sir E. Home, Bart., F.R.S.

As black surfaces become infinitely more heated than white, by exposure to the sun's rays, it has long puzzled physiologists to account for the black colour of the negro; or to develope the ends attained by that intensity of colour bestowed by the unerring hand of nature upon the inhabitants of tropical climates. In this paper Sir Everard has rendered it probable that the blistering and irritation which Europeans, not accustomed to much exposure, suffer from the scorching rays of an equatorial sun, is attributable to a peculiar effect of the solar radiant matter, unconnected with the heat which it excites. By the black colour of the negro's skin the radiant matter of the sun is absorbed, and converted into sensible or thermometric heat, and is thus disarmed of its mischievous tendency.

If we expose the back of the hand to very intense sunshine, uncovered, or covered with any thin white tissue, it becomes inflamed and blistered; but if similarly exposed under a covering of black, it suffers no inconvenience, though the temperature in the former case falls far short of that excited in the latter.

Having described a variety of experiments to this point, Sir Everard very properly takes opportunity to observe that " the same wise Providence which has given so extraordinary a provision to the negro for the defence of his skin while living within the tropics, has extended it to the bottom of the eye, which otherwise would suffer in a greater or less degree when exposed to a strong light, the retina from its transparency allowing it to pass through without injury." "That the nigrum pigmentum is not necessary for vision, but only provided as a defence against strong light, is proved by its being darker in the Negro than in the European, and lighter in fair people than in dark, and therefore lightest in those countries farthest removed from the effects of the sun." "In the monkey it is dark, and in all animals that look upwards. In all birds, exposed to the sun's rays, it is black. In fishes, the basking shark which lies upon the surface of the ocean, has a nigrum pigmentum. The turbot and skate which lie upon banks of sand in shallow water, have nigrum pigmentum. In all ruminating animals and birds of prey, there is a broad tapetum at the bottom of the eye. The owl, that never sees the sun, has no nigrum pigmentum." "The *coup de soleil* I attribute to the scorching effects of the sun's rays upon the scalp. The Egyptian ophthalmia I consider to be the effect of the sun's rays, and the glare of reflected light."

ii. *On the Magnetic Phænomena produced by Electricity, in a Letter from* Sir H. DAVY, Bart., F.R.S., *to* W. H. WOLLASTON, M.D., P.R.S.

The intimate connexion, if not the identity, of magnetism and electricity, has long been suspected, and indeed partly demonstrated; the recent discoveries of M. Oersted have completely proved the subsisting relation of these powers of matter, and have set a number of persons to work at the repetition and extension of his inquiries. In our last Number we have translated M. Biot's communication to the Parisian Academy of Sciences upon this subject, and upon a former occasion (vol. x. p. 361,) our readers have been presented with an outline of the recent discoveries connected with it. Since the date of this paper, Sir Humphry has himself extended his interesting experiments and observations; and in July last, he read a second memoir to the Royal Society, in which much that is left incomplete and undetermined in the paper now before us, is perfected and cleared up; of this we shall give an account in due course.

iii. *Communication of a singular Fact in Natural History. By the Right Hon. the* EARL of MORTON, F.R.S.

iv. *Particulars of a Fact nearly similar to that related by* LORD MORTON, *by* DANIEL GILES, Esq.

Under these mysterious titles the following facts will be found. Some years ago Lord Morton, desirous of domesticating the quagga, endeavoured to procure a male and female of that species, but succeeded only in obtaining a male; he therefore tried to breed from the male quagga and a chestnut mare, and the result was a female hybrid of decidedly mixed origin; the mare was afterwards bred from by a black Arabian horse, and this latter produce exhibits in colour and mane a striking resemblance to the quagga. It is curious enough that features not belonging to the dam should be communicated through her to the progeny of another male.

Mr. Giles says, "I will now give the best account I can of my sow and her produce." She was of Mr. Western's black and white breed. Ten years ago she was bred from by a boar of the wild breed, of a deep chestnut colour; the pigs, (her first litter,) were duly mixed, the colour of the boar being in some of them very predominant. The sow was afterwards twice bred from by Mr. Western's boars, and in both instances chestnut marks were prevalent in the litter, which in other instances had never presented any appearance of the kind.

v. *The Croonian Lecture. Microscopical Observations on the following Subjects: On the Brain and Nerves; showing that the Materials of which they are composed exist in the Blood. On the Discovery of Valves in the Branches of the Vas Breve, lying between the villous and muscular Coats of the Stomach. On the Structure of the Spleen. By Sir* EVERARD HOME, *Bart. V. P. R. S.*

This lecture will be read with much interest by those who are bold enough to fathom the depths of modern physiology, and who put that confidence in Mr. Bauer's microscopical observations which we think they deserve. He examined the optic nerve and found it to consist of many bundles of fine fibres, formed of very minute globules united by a soluble transparent jelly. "By the discovery of this transparent substance," says Sir Everard, "we become acquainted with the nature of the medullary structure of the nerves, and can form some idea of their action, which till now I confess myself to have been totally unacquainted with. The nerves as well as the retina are composed of this newly-discovered transparent substance which is very elastic and soluble in water, and globules of $\frac{1}{2800}$ and $\frac{1}{4000}$ parts of an inch in diameter. Its transparency and solubility account for its having remained concealed; and were it not coagulable, in which state it becomes opaque, its existence might even now be considered as equivocal." The brain is also, as Mr. Bauer's observations show, a conglomerate of globules and soluble mucus, the former arranged into fibres and bundles, held together by the latter. It is pervaded by blood-vessels, but the arteries never anastomose, and the veins, which are very small, are supplied with valves, and perform the office of lymphatics, carrying the absorbed matter into the superior longitudinal sinus.

Our readers are probably aware that among metaphysicians there is some slight discrepancy of opinion respecting the head-quarters of the sensorium and the seat of the soul. Physiologists have also differed respecting the functions of the various parts of the brain; after the trash that has lately been current upon these subjects, we feel particularly indebted to Sir Everard for the following sterling facts:

> "That the cortical part of the brain is the seat of memory, is an opinion I have long entertained, from finding that any continued undue pressure upon the upper anterior part of the brain entirely destroys memory, and a less degree materially diminishes it. Pressure upon the dura mater, where the skull has been trepanned, puts a temporary stop to all sense, which is restored the moment that pressure is removed; and the organ appears to receive no injury from repeated experiments of this kind having been made. In hydrocephalus, when the fluid is in large quantity, and there only remains the cortical part of the brain and pons Verolii connecting it to the cerebellum, all the functions go on, and the memory can retain passages of poetry, so as to say them by heart; but a violent shake of the head produces instant insensibility. Pressure in a slight degree upon the sinciput, produced in one case complete derangement, with violent excess of the passion of lust, both of which went off upon removing, by the crown of the trepan, the depressed bone."

A little further on, adverting to the abundance and office of the transparent mucus, Sir Everard says, "There can be no doubt that the communication of sensation and volition more or less depend upon it." Indeed, it is evident that those functions cannot be ascribed to any individual component of the brain and nerves, but belong to them as entire structures.

The remainder of this part of the lecture is taken up in attempting to show that the above-mentioned mucus exists ready formed in the blood, and that it is the medium "by which the colouring matter is attached to the surface of the red globules," and that fat may exist in the blood.

Having dismissed the structure of the brain, the next portion of this lecture is devoted to a subject which some of our readers will perhaps consider as of paramount importance; namely, to the provision for carrying off the fluids taken into the stomach, whenever the quantity or quality interferes with the process of digestion.

> "To do this by the route of the thoracic duct, was not only too circuitous to correspond with the general simplicity of the operations of nature, but was mixing these heterogeneous liquids in too crude a state, with the general circulation of the blood. That there was some unusual mode of conveying fluids from the stomach to the urinary bladder, I have upon a former occasion established, since they arrived there when both the pylorus and thoracic duct were tied up, and the spleen was removed out of the body; but till the fact of valvular vessels supplying the office of absorbents was ascertained, any opinion respecting the route of fluids from the stomach, must continue to be entirely hypothetical."

Our author then proceeds to demonstrate the existence of such vessels, and to describe their situation and appearance, aided by engravings of Mr. Bauer's admirable drawings.

"To show the course of the absorbed fluids, as well as to give a clear idea of every thing connected with so *important* a discovery, a drawing of the spleen, the vas breve, and cardiac portion of the stomach, is annexed [Pl. IV;] and as the trunk of the splenic vein forms one of the trunks of the vena portæ, the liquids are directly carried to the liver, forming a part of the materials employed in producing the bile; the remainder only returning by the vena cava to the heart.

"This additional quantity of liquids passing along the splenic vein, accounts for its being five times the size of the artery, as well as for the blood in that vein having a greater proportion of serum than the blood in any other, which has been long asserted, and which I found by actual experiment to be the case; but being unable to account for it, as I can now, I was willing to admit that the mode of measuring might be erroneous."

The remaining section of this lecture is dedicated to the structure and uses of the spleen. It is shown to consist of blood-vessels, between which there is no cellular membrane, the interstices being filled with serum, and with the colouring matter of the blood from the lateral orifices in the veins when these vessels are distended, which serum is afterwards removed by the absorbents belonging to the organ, and carried into the thoracic duct by a very large absorbent trunk; so that from this mechanism "the spleen appears to be a reservoir for the superabundant serum, lymph globules, soluble mucus, and colouring matter, carried into the circulation immediately after the process of digestion is completed."

vi. *On two new Compounds of Chlorine and Carbon, and on a new Compound of Iodine, Carbon, and Hydrogen.* By Mr. M. Faraday, *Chemical Assistant in the Royal Institution.*

In a former number of our Journal we have abstracted the material facts contained in this paper*, (vol. x., p. 386;) our readers will there observe that the author has described the properties of two new compounds of chlorine and carbon; in conjunction with Mr. Phillips, he has more recently ascertained the existence of another combination of the same elements in different proportions, so that chlorine and carbon which a year ago were not known to be susceptible of chemical union, are now shown to constitute three distinct compounds.

The various direct and collateral researches by which the nature and properties of these bodies are determined and illustrated, are amply and explicitly given in the paper before us, which we consider as a truly valuable contribution to chemical science; Mr. Faraday is simple and perspicuous without being superficial or hasty; he has sometimes handled the more abstract parts of chemistry, but has always kept clear of that affected jargon which characterizes a set of writers that may be designated the *Berzelian School*, and who are fond of mystifying and obscuring the simplest facts and doctrines of the science, by symbols and algebra: nothing is easier than to write a, b, c, instead of 1, 2, 3; to represent carbon by a great C, and chlorine by a little c, and then to call the chloride of carbon $C + c$, and the bichloride $C + c^2$, and so forth; but what then? chemistry is not yet ripe for these innovations; and when it is, they must come from abler heads than those of their present propounders.

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* It has also been published at length in Mr. Phillips's *Annals of Philosophy*.

vii. *An Account of the Comparison of various British Standards of linear Measure.* By Captain Henry Kater, F.R.S.

Captain Kater's communications to the Royal Society have of late been numerous and important; they are not, generally speaking, of a popular cast, and have not therefore attracted that general notice which is bestowed upon productions carrying more sail with less ballast; independently too of their intrinsic value, they are written in a style peculiarly clear and intelligible. Nothing is hastily thrust forward in an unfinished state, with the mere view of pre-occupying the ground; no immature opinions are started for the purpose of anticipating the results of others engaged in similar pursuits *.

The paper before us, one only of several connected with the same subject, contains the details of a laborious series of inquiries connected with the Reports of the Commissioners of Weights and Measures; for the third and last of which we refer to the last volume of this *Journal*, p. 378. Our readers will also there find the results of some further inquiries instituted by Captain Kater, relative to the re-measurement of Sir George Shuckburgh's cube, sphere, and cylinder, the details of which he has also given to the Royal Society, whose Transactions are certainly the fit repository for these and all similar documents.

viii. *An Account of the Urinary Organs and Urine of two Species of the genus Rana.* By John Davy, M.D. F.R.S.

The bull-frog, *(rana taurina*, Cuvier,*)* and the brown toad, *(bufo fuscus*, Laurenti,) are the two species to which the author's observations are limited in this paper, which is dated from Ceylon: the former inhabits the lake of Colombo; the latter abounds in the houses and streets of the Pettah. The specific gravity of the urine of the bull-frog is about 1003, and common salt, with a little phosphate of lime, and a trace of urea, are the only ingredients detected in it. The specific gravity of the urine of the brown-toad is 1008; its constituents similar to the above, but the proportion of urea more considerable. For the anatomical facts we must refer to the paper itself; the following physiological observations are important in relation to the treatment of urinary disorders; as such we quote them at length.

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* These remarks are suggested to us by the sweeping and unbounded surmises in which some cotemporary *sçavans* are accustomed to indulge, for

> "Perhaps additional facts are not required to prove, that the secretion of the kidneys of animals depends more on the intimate and invisible structure of these organs, than on the kind of food the animals consume; were such facts wanting, there would be no difficulty in furnishing them. How different is the urine of the brown-toad and that of any species of small lizards! yet flies are the favourite and common diet of both animals. Other remarkable instances might be mentioned, of similarity of diet and difference of urinary secretion; and, *vice versâ*, instances might be afforded of difference of diet and similarity of urine: I will mention one only; it is that of parrots and snakes; their urine, as I have found, being much the same, consisting chiefly of uric acid, though their diet is altogether different, the birds feeding entirely on vegetable matter, and the reptiles entirely on animal matter. But let me not be supposed to maintain that the urinary secretion depends entirely on the organ, quite independent of the nature of the food or of the blood, from which the elements of the urine are derived. It appears to be pretty satisfactorily proved, that *cæteris paribus*, there is a certain relation between the nature of the food and of the urine. Whilst this has been generally admitted, the relation between the organ and the secretion has been less insisted on, though perhaps not less curious and deserving of attention."

IX. *An Account of a Micrometer made of Rock Crystal.* By G. DOLLOND, F.R.S.

Rock crystal has been applied to telescopes in various ways for the purposes of micrometrical measurements: Mr. Dollond's improvement consists in making a sphere or lens from a piece of rock crystal, and adapting it to a telescope in the place of the usual eye-glass, and from its natural double refracting property rendering it useful as a micrometer.

The advantages of this application of the crystal consist in the saving of the time required to find the proper angle for cutting it, and working its surfaces with sufficient accuracy; in being able to take the angle on each side zero, without reversing the eye-tube, and also to take intermediate angles

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the mere purpose, as we presume, of laying claim to the merit of future discoveries, to which they are less entitled than the Marquis of Worcester to the invention of the steam-engine, or Roger Bacon to that of gunpowder.

between zero and the greatest separation of the images without exchanging any part of the eye-tube, it being only required to move the axis in which the sphere is placed. It also possesses the property of an eye-tube or lens not intended for micrometrical purposes, for when the axis of the crystal is parallel to that of the object glass, only one image will be formed. A plate is annexed to this paper, illustrative of the contrivance.

x. *The Bakerian Lecture: on the best kind of Steel and Form for a compass-needle.* By Captain Henry Kater, F.R.S.

On the return of the expedition under the command of Captain Ross, which sailed in search of a north-west passage, it appeared that the compasses became nearly useless from the diminution of directive force, in consequence of the vicinity of the magnetic pole. Some of the azimuth compasses then employed were of Captain Kater's invention; he was therefore naturally anxious that the next expedition, (under the command of Lieutenant Parry,) should be furnished with instruments combining as much power and sensibility as possible; with this intention, our author undertook the experiments which form the subject of the valuable practical document before us, and from which he draws the following inferences:

"1. That the best material for compass-needles is *clock spring;* but care must be taken in forming the needle to expose it as seldom as possible to heat, otherwise its capability of receiving magnetism will be much diminished.

"2. That the best form for a compass-needle is the *pierced rhombus,* in the proportion of about five inches in length to two inches in width, this form being susceptible of the greatest directive force.

"3. That the best mode of tempering a compass-needle is, first to harden it at a red heat, and then to soften it from the middle to about an inch from each extremity, by exposing it to a heat sufficient to cause the blue colour which arises again to disappear.

"4. That in the same plate of steel of the size of a few square inches only, portions are found varying considerably in their capability of receiving magnetism, though not apparently differing in any other respect.

"5. That polishing the needle has no effect on its magnetism.

"6. That the best mode of communicating magnetism to a needle, appears to be by placing it in the magnetic meridian, joining the opposite poles of a pair of bar magnets (the magnets being in the same line), and laying the magnets so joined flat upon the needle with their poles upon its centre; then having elevated the distant extremities of the magnets, so that they may form an angle of about two or three degrees with the needle, they are to be drawn from the centre of the needle to the extremities, carefully preserving the same inclination, and having joined the poles of the magnets at a distance from the needle, the operation is to be repeated ten or twelve times on each surface.

"7. That in needles from five to eight inches in length, their weights being equal, the directive forces are nearly as the lengths.

"8. That the directive force does not depend upon extent of surface, but in needles of nearly the same length and form, is as the mass.

"9. That the deviation of a compass needle occasioned by the attraction of soft iron, depends, as Mr. Barlow has advanced, on extent of surface, and is wholly independent of the mass, except a certain thickness of the iron, amounting to about two-tenths of an inch, which is requisite for the complete developement of its attractive energy."

xi. *Notice respecting a Volcanic Appearance in the Moon, in a Letter addressed to the President.* By Captain Henry Kater, F.R.S.

We have heard of volcanos in the moon, but when we consider the fallacies to which observations of her surface are liable, even when made with the most experienced eyes and perfect instruments, we are induced to doubt the identity of most of these appearances with terrestrial volcanic phenomena, and to applaud Captain Kater for the caution shewn in the title of this letter, where he calls the luminous spot which he discovered, not a volcano, but a *volcanic appearance*. On Sunday evening, the 4th of February, 1821, the moon being two days old, and the evening clear, Captain Kater observed a luminous spot in the dark part of her orb; its appearance was that of a small nebula, subtending an angle of three or four seconds, and of variable brightness. "A luminous point," says our author, "would suddenly appear in its centre, and as suddenly disappear, and these changes would sometimes take place in the course of a few seconds. On the 5th, 6th, and 7th, it was again observed, but not afterwards." We should have mentioned that the telescope which Captain Kater used was a Newtonian, of 6¼ inches aperture, with a power of seventy-four. An engraving accompanies this paper.

xii. *A further Account of Fossil Bones, discovered in Caverns, enclosed in the Limestone Rocks at Plymouth.* By Joseph Whidbey, Esq.

We have already given the essence of this paper; it includes Mr. Whidbey's account of the situation of the caverns, and Sir Everard Home's catalogue of the relics, which we think are not quite properly called fossil bones; this, however, might easily be determined by chemical analysis. They are deposited in the museum of the College of Surgeons.

xiii. *On the Aëriform Compounds of Charcoal and Hydrogen, with an account of some additional Experiments on the Gases from Oil and from Coal.* By William Henry, M.D. F.R.S., &c.

The labours of Dr. Henry have tended more perhaps than those of any other individual to elucidate the nature and properties of those compound inflammable gaseous bodies of which charcoal and hydrogen are the principal constituents, and which are evolved during the destructive distillation of various vegetable and animal products. Much, however, still remains to be effected; it remains to be ascertained whether there are two or more definite combinations of carbon and hydrogen, or whether the supposed varieties are merely mixtures of one definite compound (olefiant gas) with variable proportions of pure hydrogen; the exact nature, too, of the gases from coal and oil is by no means made out; indeed we do not know whether the anomalies which they present to the analyst, are referable to the presence of some highly inflammable and volatile oily vapour, or to the existence of some hitherto unexamined compound of hydrogen and carbon; we are inclined to lean to the former opinion, for the illuminating power of the gas from oil is diminished by violent compression; by subjection to intense cold; by long exposure to water; and by passing it through alcohol; and in all these cases it loses oily matter, and requires consequently less oxygen for its perfect combustion. Whether there be two definite compounds of carbon and hydrogen, or one only, is a question which should be determined before the other ramifications of this inquiry can be satisfactorily traced; in the Bakerian lecture for 1820, Mr. Brande has advanced the former opinion; Dr. Thomson was the first to oppose it in the late edition of his *System*; but we have already canvassed and quashed his criticism*.

Dr. Henry also argues in favour of the existence of light hydrocarbonate; such parts of his argument as hinge upon Dr. Thomson's authority we shall not notice; the others are chiefly the following. The gas obtained from the clayey bottom of a stagnant pool contained $\frac{1}{20}$ its volume of carbonic acid, and $\frac{1}{15}$ of nitrogen; the pure portion required two volumes of oxygen for combustion, and gave one volume of carbonic acid; its specific gravity was about 556. Now as these results agree with those obtained by Sir H. Davy in his analysis of *fire-damp*, Dr. Henry says there appears no reason for refusing to consider carburetted hydrogen (light) as a true chemical compound, and were any additional argument necessary to establish its definite nature, he observes that it might be derived from the action of water, which absorbs it in a constant proportion, and again evolves it unaltered when heated. But with great deference for Dr. Henry's opinion, we suspect that this latter point has not been sufficiently verified, and even if admitted, we doubt whether it would justify the inference which is here drawn from it.

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* Vol. XI., page 145.

Dr. Henry then says, that "the process by which carburetted hydrogen gas is evolved in natural operations is no doubt the decomposition of water, and admits of being explained on the atomic theory of Mr. Dalton, by supposing two atoms of charcoal to act at once on two atoms of water," *&c.* Now we confess that we have great doubts as to the above origin of carburetted hydrogen gas, and rather suspect that the decomposition of vegetable and animal matter, is its principal, if not exclusive, source in nature; arising perhaps in coal-mines, from the action of whin dikes upon the coal, as suggested by Dr. Hutton; and derived in stagnant pools from the decomposition of the organic relics contained in the mud. As to the absence of carbonic oxide in the products of stagnant water, we would, in the first place, ask, has its absence been satisfactorily ascertained? and, secondly, admitting that it does not exist, why should we expect the presence of a compound which neither fermentation, putrefaction, nor respiration produce, and which is only sparingly and equivocally generated during the action of heat on pit-coal?

The next portion of Dr. Henry's paper relates to the action of chlorine on carburetted hydrogen; a subject, putting the purity of the carburetted hydrogen out of the question, so full of difficulties, so mixed up with the joint agency of light, heat, and water, upon the mixed gases, that we cannot presume here to review it in all its bearings, and shall therefore rest content with thanking Dr. Henry for the new facts which he has stated in relation to it.

In his "*Experiments on the gas from oil,*" Dr. Henry has shewn that the gas obtained at different times from oil of the same quality is by no means uniform in composition. The readers of this Journal will probably require an apology for this mere mention of the "oil question;" but the following remark, coming from a person of Dr. Henry's experience and accuracy, we think weighty in relation to it. "So far," he says, "as my experience goes, no temperature short of ignition is sufficient for the decomposition of oil into permanent combustible gases; but the lower the heat that is employed, provided it be adequate to the effect, the heavier and more combustible is the gas, and the better suited to artificial illumination." This paragraph is also important to the manufacturer of oil gas for domestic use, but least he should be led into error, we would observe that although Dr. Henry is perfectly correct, it still does not answer to the manufacturer to obtain the gas in Messrs. Taylors' stoves, at the lowest possible temperature, that is, a dull red heat, for at that temperature, much empyreumatic condensible vapour is produced, and the bulk of the gas proportionably diminished: and though it be perfectly true that the increased bulk of the gas obtained at higher temperatures is partly referable to the decomposition, and consequent expansion of the olefiant portion, and therefore attended by a certain diminution of illuminating power, still, as far as economy is concerned, it will be found advantageous to make oil gas at a bright, or cherry-red, heat.

The next section of Dr. Henry's paper contains the results of his experiments on coal gas, which are conducted with his usual skill and patience, but the analyses are so much affected by the questions discussed in the concluding section, that we shall prefer employing our remaining space in calling the reader's attention to the author's "*inferences respecting the composition of that part of the gas from coal and oil, which is condensed by contact with chlorine.*" By a variety of experiments, Dr. Henry considers it as proved, that that portion of oil and coal gas, which is condensible by chlorine, (light being excluded), is of a specific gravity of 1.4 to 1.2, and consequently, that it is not pure olfiant gas. Again, both Dr. Henry and Mr. Dalton, agree that the portion of oil and coal gas condensible by chlorine, requires for its entire combustion not three volumes of oxygen (which would indicate olefiant gas,) but four volumes and a half; so that as our author himself remarks—

> "It is evident from these facts that the aëriform ingredient of oil gas and coal gas, which is reducible to a liquid form by chlorine, is not identical with the olefiant gas obtained by the action of sulphuric acid on alcohol, but considerably exceeds that gas in specific gravity and combustibility. Two views may be taken of its nature; for it may either be a gas *sui generis*, hitherto unknown, and constituted of hydrogen and charcoal in different proportions from those composing any known compound of those elements; or it may be merely the vapour of a highly volatile oil, mingled in various proportions with olefiant gas, carburetted hydrogen, and the other combustible gases. Of these two opinions, Mr. Dalton is inclined to the first, considering it as supported by the fact that oil gas, or coal gas, may be paſsed through water, without being deprived of the ingredient in question; and that this anomalous elastic fluid is absorbed by agitation with water, and again expelled by heat or other gases, unchanged as to its chemical properties, as we have both satisfied ourselves by repeated experiments. On the other hand, I have found that hydrogen gas, by remaining several days in narrow tubes in contact with fluid naphtha, acquires the property of being affected by chlorine, precisely as if it were mixed with a small proportion of olefiant gas; and I am informed by Dr. Hope, that oil gas, when forcibly compressed in Gordon's portable gas lamp, deposits a portion of a highly volatile essential oil. The smell also of the liquid which is condensed on the inner surface of a glass receiver, in which oil gas or coal gas has been mixed with chlorine, denotes the presence of chloric ether, evidently however mingled with the odour of some other fluid, which seems to me to bear most resemblance to that of spirit of turpentine. This part of the subject is well worthy of further investigation; but having devoted to the inquiry all the leisure which I am now able to command, I must remain satisfied at present with such conclusions as are safely deducible from the foregoing investigation."

We entirely agree with Dr. Henry that this subject deserves further investigation, and as no one can be better qualified for its prosecution than himself, we trust that he will resume and complete that which he has so ably commenced. It is not only interesting as an abstract chemical inquiry, but may also prove important in its relations to the economy of gas illumination.

C.

ART. XV. ASTRONOMICAL AND NAUTICAL COLLECTIONS. No. VII.

i. *An Essay on the easiest and most convenient Method of calculating the Orbit of a Comet from Observations. By* WILLIAM OLBERS, M.D. 8vo. *Weimar*, 1797.

[Continued from Vol. XI. p. 182.]

SECTION II.

On some Equations of the First and Second Order, which have been proposed for determining the Equations of Comets.

§ 40.

Now the chord k'' being (§ 7) $= \surd\,([x''' - x']^2 + (y''' - y')^2 + (z''' - z')^2)$; if we develope this formula, and remember that $r'^2 = x'^2 + y'^2 + z'^2$, and $r'''^2 = x'''^2 + y'''^2 + z'''^2$, we shall find $k'' = \surd\,(r'^2 + r'''^2 - 2x'x''' - 2y'y''' - 2z'z''')$: and since we had $x' = \varrho' \cos \alpha' - R' \cos A'$, $y' = \varrho' \sin \alpha' - R' \sin A'$. $z' = \varrho' \,\mathrm{tang}\, \beta'$; and $x''' = M\varrho' \cos \alpha''' - R''' \cos A'''$, $y''' = M\varrho \sin \alpha''' - R''' \sin A'''$, $z''' = M\varrho' \,\mathrm{tang}\, \beta'''$; we obtain $x'x''' + y'y''' = R'R''' \cos (A''' - A') - \varrho' R''' \cos (A''' - \alpha') - M\varrho' R' \cos (A' - \alpha'') + M\varrho'^2 \cos (\alpha''' - \alpha';)$ and $z'z''' = M\varrho'^2 \,\mathrm{tang}\, \beta' \,\mathrm{tang}\, \beta'''$; so that the whole equation becomes $k''^2 = r'^2 + r'''^2 - 2\,R'R''' \cos (A''' - A') + 2\,\varrho' R'' \cos (A''' - \alpha') + 2M\varrho' R' \cos (A' - \alpha'') - 2\,M\varrho'^2 \cos (\alpha''' - \alpha') - 2\,M\varrho'^2 \,\mathrm{tang}\, \beta' \,\mathrm{tang}\, \beta'''$: for which we may write $k = \surd\,(F + G\varrho' + H\varrho'^2)$.

§ 41.

Now T being the time between the first and third observation, we have, from LAMBERT's very elegant theorem, $T = \frac{1}{12\,m}\left([r' + r'' + k')^{3} - (r' + r''' - k'')^{\frac{3}{2}}\right)$. But if we substituted, in this equation, the values of r', r''' and k'', we should arrive at an equation of enormous difficulty: it might, however, be reduced to the 12th degree by substituting for the equation of LAMBERT the approximation of DUSE'JOUR, $T^2 = \frac{(r' + r''')\,k''^2}{4f}$; and even to the 6th, by supposing $\frac{r' + r'''}{2} = \surd\,\frac{r'^2 + r'''^2}{2}$, which is, however, only at all admissible when r' and r''' differ but little from each other, and k'' and T are very small. There is, however, no occasion for these awkward abbreviations; for although the value of ϱ' cannot be found immediately from Lambert's formula, yet it may be obtained by a few easy trials, since we have three equations,

$$r' = \sqrt{(R'^2 - 2R' \cos(A' - \alpha')\varrho' + \sec^2\beta'\varrho'^2)}$$

$$r''' = \sqrt{(R'''^2 - 2R''' \cos(A''' - \alpha''')M\varrho' + \sec^2\beta''' M^2\varrho'^2)}$$

$k'' = \sqrt{(F + G\varrho' + H\varrho'^2)}$; in all which the coefficients of ϱ' are known magnitudes, expressed in numbers; so that we have only to assume a value for ϱ', and we obtain those of r' r''' and k'' by the extraction of the square root only. From these we may find without difficulty, by means of the table of the descent towards the sun in a parabola, or by an easy direct reckoning, the time that ought to elapse between the observations, according to the assumed value of ϱ'. This time, compared with the observed time, immediately shews whether we ought to increase or diminish the value of ϱ', in order to come nearer to the observation. In this manner we approximate rapidly to the truth, and may at last employ a simple interpolation. It will seldom be necessary to make more than four, or at most five, suppositions; and the first of them will not require any accuracy of calculation: at least the determination of the true value of ϱ' from these three equations will always be much more convenient than the solution of a single equation of the 6th degree.

§ 42.

As soon as the value of ϱ' is determined, the determination of all the elements readily follows: for the computation gives us at once r', r''', ϱ', and $\varrho''' = M\varrho'$. Now if we call the heliocentric latitudes, in the first and third observations, λ' and λ''', we have $\sin\lambda' = \frac{\tan\beta'\varrho'}{r'}$, and $\sin\lambda''' = \frac{\tan\beta'''\varrho'''}{r'''}$. If also we put ε', ε''' for the heliocentric elongations of the comet from the earth, we find $\sin\varepsilon' = \frac{\varrho'\sin(A' - \alpha')}{r'\cos\lambda'}$, $\sin\varepsilon''' = \frac{\varrho'''\sin(A''' - \alpha''')}{r''\cos\lambda'}$ whence we obtain the two heliocentric longitudes, which may be called C' and C'''. If now we put $\cot \omega = \dfrac{\text{tang}\, \lambda'''}{\text{tang}\, \lambda' \sin (C''-C')} - \cot (C'''-C')$, we shall have ω the distance of the comet in longitude, at the time of the first observation, from the ascending node; and consequently $C' - \omega$ the longitude of the node. The inclination of the orbit is obtained from the formula $\text{tang}\, i = \dfrac{\text{tang}\, \lambda'}{\sin \omega}$. For the two heliocentric distances of the comet from its node in the plane of its orbit, u', u''', we have $\cos u' = \cos \lambda' \cos \omega$, and $\cos u''' = \cos \lambda''' \cos (C'''-C'+\omega)$; and $u'''-u'$ is the difference of the two true anomalies. Now if we make φ the true anomaly, in the first observation φ, we have, from the known properties of the parabola, $\text{tang}\, \frac{1}{2} \varphi = \cot \dfrac{u'''-u'}{2} - \text{cosec}\, \dfrac{u'''-u'}{2} \cdot \sqrt{\dfrac{r'}{r'''}}$, whence we have the longitude of the perihelium; and the distance π at the perihelium is found $= r' \cos^2 \frac{1}{2} \varphi$; the time may also readily be found either by direct computation, or by a table. [Or since we must previously have determined the values of r', r''' and k'', we may obtain that of π from the equation A, Note 3; $= \frac{1}{4} \cdot \dfrac{k''^2 - (r'''-r')^2}{r'+r''' - \sqrt{(r'+r''')^2 - k''^2}}$, and then $\cos \varphi = \dfrac{2\pi - r'}{r'} = \dfrac{2\pi}{r'} - 1$. The longitude of the node may also be deduced from its tangent $\dfrac{y}{x} = \dfrac{y'z''' - y'''z'}{x'z''' - x'''z'}$, Note 2. TR.] NOTE *communicated* by the AUTHOR. [It may be somewhat more convenient to employ the formula $\text{tang}\left(\omega + \dfrac{C''-C'}{2}\right) = \dfrac{\sin (\lambda''' + \lambda')}{\sin (\lambda''' - \lambda')} \text{tang}\left(\dfrac{C'''-C'}{2}\right)$.]

§ 43.

It is natural, as soon as ϱ' is found, to have the curiosity to calculate all the elements of the orbit, although there is no actual necessity to do so, until further corrections have been introduced; and, as LAPLACE has justly remarked, in so long a computation, we ought to spare ourselves every unnecessary labour. We might therefore be contented to determine either the longitude of the node and the inclination of the orbit, or the time and distance of the perihelium, according to the mode of correction which we might prefer. Thus if χ be $= u'' - u'$, or the angle formed by the two positions of the revolving radius, we have $\cos\chi = \frac{r'^2 + r'''^2 - k''^2}{2r'r'''}$, and thence φ by the formula $\text{tang}\,\frac{1}{2}\varphi = \cot\frac{1}{2}\chi - \sqrt{\frac{r'}{r'''}} \cdot \frac{1}{\sin\frac{1}{2}\chi}$; which enables us to determine the time and distance. We may also find φ more directly, since $\sin^2\frac{1}{2}\chi = \frac{k'' - (r''' - r')^2}{4\,r'''r'}$ and $\cos^2\frac{1}{2}\chi = \frac{(r''' + r')^2 - k''^2}{4r'''\,r'}$, whence $\cot^2\frac{1}{2}\chi = \frac{(r''' + r')^2 - k''^2}{k''^2 - (r' - r')^2}$, consequently $\text{tang}\,\frac{1}{2}\varphi =$ $\frac{\sqrt{([r''' + r']^2 - k''^2)} - 2r'}{r\,(k''^2 - (r''' - r')^2}$.

§ 44.

It will be convenient to recapitulate the formulæ immediately necessary for the computation, that the whole may be found together. We first find $m = \frac{\text{tang}\,\beta''}{\sin(\text{A}'' - \alpha')}$ then $\text{M} =$ $\frac{(m\sin(\text{A}' - \alpha') - \text{tang}\,\beta')\,t''}{(\text{tang}\,\beta''' - m\sin[\text{A}'' - \alpha''''])\,t'}$, and the coefficients of

$r'^2 = \text{R}'^2 - 2\,\text{R}'\cos\,(\text{A}' - \alpha')\,\varrho' + \sec^2\beta'\varrho'^2$

$r'''^2 = \text{R}'''^2 - 2\,\text{R}\cos\,(\text{A}''' - \alpha''')\,\text{M}\varrho' + \sec^2\beta''\;\text{M}^2\varrho'^2$

$k''^2 = r'^2 + r'''^2 - 2\,\text{R}'\,\text{R}'''\cos\,(\text{A}''' - \text{A}') + 2\,\text{R}'''\cos\,(\text{A}''' - \alpha')\,\varrho'$ $+ 2\,\text{M}\,\text{R}'^2\cos\,(\text{A}' - \alpha''')\,\varrho' - 2\,\text{M}\cos\,(\alpha''' - \alpha')\,\varrho'^2 - 2\,\text{M}\,\text{tang}\,\beta,$ $\text{tang}\,\beta'''\,\varrho'^2$; hence we obtain ϱ' by a few trials, and from it all the other elements of the orbit, by means of the formulas in §. 42 and 43.

§ 45.

Even a superficial comparison of this method with any other that has been hitherto proposed, will be sufficient to show its superior conciseness and convenience. It has also the advantage of being universally applicable, whenever we have three observations of a comet. It is true that the proportion, in which the chords are supposed to be divided, is not mathematically correct; but this source of error is by no means very material: Euler and Lambert have supposed the same for the orbit of the comet; I have only extended the supposition to that of the earth, and by this extension the inaccuracy will never be considerably increased, and frequently even diminished. None of the direct methods which have been proposed is so accurate; for they all take for granted, either tacitly or expressly, that the portion of the comet's orbit is a right line described uniformly; or if the arcs be assumed, with Laplace, so small that the supposition creates no error, these arcs must be determined by means of an awkward interpolation. I shall also show, in the next section, how easily the error arising from the imperfection of the supposition may be corrected.

§. 46.

This method of calculation may, however, be more completely illustrated by an example; and I shall take for this purpose that of the comet of 1769, partly because the elements of its orbit are so well established, and partly because it has been the most frequently employed for an example of other methods. The observations are from Pingré's Cométographie.

Times			α			β			
Sept. 4	14h	80°	56′	11″	17°	51′	39″	S
8	14	101	0	54	22	5	2	
12	14	124	19	22	23	43	55	

For these three observations we have

	A		Log R
162°	42′	5″	.003132
166	35	31	.002665
170	20	20	.002184

Consequently $t' = t'' = 4$ days, $\frac{t''}{t'} = 1$, and $T = 8$ days.

The computation for M will stand thus

	Log tang β''	9.608237
	$-$Log sin $(A''-\alpha'')$	9.959299
	$=$Log m	9.648938
$+\Big\{$	Log sin $(A''-\alpha')$	9.998750
	Log sin $(A''-\alpha''')$	9.827766
$=\Big\{$	Log m sin $(A''-\alpha')$	9.647688
	Log m sin $(A''-\alpha''')$	9.476704

Tang β'''	.43963
m sin $(A''-\alpha''')$	.29971
Tang $\beta'''-m$ sin $(A''-\alpha''')$	.13992
m sin $(A''-\alpha')$	.44431
Tang β'	.32221
m sin $(A''-\alpha')-$tang β'	.12210

Log .12210	9·086716
$-$Log .13992	9.145880
$=$Log M	9.940836

We next compute the formulas
$r'^2=R'^2-2\,R'\cos(A'-\alpha')\,\varrho'+\sec^2\beta'\,\varrho'^2$, and
$r'''^2=R'''^2-2\,R'''\cos(A'''-\alpha''')\,M\,\varrho'+\sec^2\beta'''\,M^2\,\varrho'^2$, which become $r'^2=1.01453-0.28854\,\varrho'+1.10393\,\varrho'^2$
$r'''^2=1.01011-1.21482\,\varrho'+.90869\,\varrho'^2$; then for the chord, $k^2=r'^2+r'''^2-2\,R'\,R'''\cos(A'''-A')+2\,R'''\cos(A'''-\alpha')\,\varrho'+2\,R'\cos(A'-\alpha''')M\varrho'-2\,M\cos(\alpha'''-\alpha')\,\varrho'^2-2\tan\beta'\tan\beta'''\,M\,\varrho'^2$; and the calculation stands thus

(1) Log R'	.003132	(2) Log R'''	.00218
$+$ Log R'''	.002184	$+$ Log cos $(A'''-\alpha')$	7.89400
$+$ Log cos $(A'''-A')$	9.995976	$=$ Log .007875	7.89618
$=$ Log 1.00298	0.001292		

(3) Log M	9.940836	(4) Log M	9.940836
$+$ Log R'	.003132	$+$ Log cos $(\alpha'''-\alpha')$	9.861377
$+$ Log cos $(A'-\alpha''')$	9.894274	$=$ Log .63418	9.802213
$=$ Log .689035	9.838242		

$$\begin{array}{ll}
(5)\ \text{Log M} & 9.940836 \\
+\ \text{Log tang}\ \beta' & 9.508173 \\
+\ \text{Log tang}\ \beta''' & \underline{9.643090} \\
=\ \text{Log}\ .12362 & 9.092099
\end{array}$$

Then adding twice each of these numbers to $r'^2+r'''^2$, we have

$$\begin{array}{ll}
r'^2+r'''^2= & 2.02464-1.50336\,\varrho'+2.01262\,\varrho'^2 \\
 & \underline{-2.00596+1.39382\,\varrho'-1.51560\,\varrho'^2} \\
k''^2= & .01868-\ .10954\,\varrho'+\ .49702\,\varrho'^2
\end{array}$$

Our three equations therefore become

$$\begin{aligned}
r''' &= \sqrt{(1.01011-1.21482\,\varrho'+\ .90869\,\varrho'^2)} \\
r' &= \sqrt{(1.01453-\ .28854\,\varrho'+1.10393\,\varrho'^2)} \\
k'' &= \sqrt{(\ .01868-\ .10954\,\varrho'+\ .49702\,\varrho'^2)}.
\end{aligned}$$

In which if we take $\varrho'=1$, we have $r'=1.40...$, $r'''=.84...$, and $k''=.62...$, giving the time in which the area is described 26.88 days; while the observation gives only 8: consequently this value of ϱ' is much too great.

We will therefore take $r=.5$; then r' will be 1.07, $r'''=.80$, and $k''=.297$, and the time 11.83 days, which is still too great. We may next try $\varrho'=\frac{1}{3}$; giving $r'=1.02$, $r'''=.84$, $k''=.194$, and the time 7.79 days, or somewhat too small. Hence it may be inferred that the true value of ϱ' cannot differ much from .35. We may therefore compute more accurately for .345 and .350, and we shall find

$$\begin{array}{ll}
\varrho'=.345 & \varrho'=350 \\
r'=1.02294 & r'=1.02409 \\
r'''=.83616 & r'''=.83441 \\
k''=\ .20012 & k''=.20304 \\
\text{T}=\ 7.9271 & \text{T}=8.0410
\end{array}$$

Consequently the error of the former supposition is $-.0729$, of the latter $+.0410$; whence the true value of ϱ' is .34820; and by an easy interpolation we find $r'=1.02367$, $r'''=.83504$; and Log $\varrho'''=$Log M $\varrho'=9.482665$.

§ 47.

In order to determine the whole orbit, we obtain the heliocentric latitudes from the formula.

$\text{Sin}\ \lambda = \frac{\varrho \text{ tang } \beta}{r}$; whence $\lambda' = 6°.17'.34''$, $\lambda''' = 9°.12'.19''$. For the elongations from the earth, we have

$\text{Sin}\ \varepsilon = \varrho \frac{\sin(A - \alpha)}{r \cos \lambda}$; hence $\varepsilon' = 19°.47'.47''$, and $\varepsilon'' = 15°.25'.16''$: consequently the heliocentric longitudes of the comet are $C' = 0.^s 2°.29'.5''$, and $C''' = 0^s.5°.54'.3''$. By means of the expression $\cot\ \omega = \frac{\text{tang}\ \lambda'''}{\text{tang}\ \lambda' \sin(C''' - C')} - \cot\ (C''' - C')$, we find $\omega = 7°.11'.45''$; whence the longitude of the descending node, since the latitudes are south, will be $C' - \omega = 0^s.2°.29'.5'' - 7°.11'.45'' = 11^s.25°.18'.7''$. The inclination is found, from the equation $\text{tang}\ i = \frac{\text{tang}\ \lambda'}{\sin \omega}$, $= 41°.21'.30''$. In the next place, we have $\cos u' = \cos \lambda' \cos \omega$, and $\cos u''' = \cos \lambda''' \cos (C''' - C' + \omega)$, consequently $u' = 9°.32'.54''$, $u''' = 14°.0'.40''$, and $u''' - u' = \chi = 4°.27'.46''$. Hence we find φ, taking the third observation, because it is nearer to the sun, by means of the formula $\text{tang}\ \frac{1}{2} \varphi = \cot \frac{1}{2} \chi - \sqrt{\frac{r'''}{r'}} \cdot \frac{1}{\sin \frac{1}{2} \chi}$; which gives $\frac{1}{2} \varphi = 67°.56'.12''$, and the true anomaly of the comet in the third observation $135°.52'.24''$. If we add to φ the distance of the comet from the ☋, or u''', $= 14°.0'.40''$, we obtain the distance of the perihelium from the descending node $= 149°.53'.4''$; consequently, the longitude of the perihelium is $4^s.25°.11'.11''$. The distance π at the perihelium is $r''' \cos^2 \frac{1}{4} \varphi = .11782$: whence we find the time from the third observation to the perihelium, $=$ 24d.20h.22m.; and the time of the perihelium October 7, 10h.22m.

[*Note.*—It must here be remarked that it has been usual with all astronomers, since the time of Halley, to apply the term *Longitude of the Perihelium* to the sum of two angles lying contiguous to each other in different planes; one the longitude of the node, reckoned on the ecliptic, the other the distance of the perihelium from the node, measured on the plane of the orbit. To reduce the longitude of the perihelium to the ecliptic, in the common sense of the term, would be of no practical advantage. TR. From a letter of the AUTHOR.]

§ 48.

The elements thus found are these:

Longitude of the ☊ 5ˢ.25°.18′.7″
Inclination of the orbit 41°.21′.30″
Longitude of the perihelium 4ˢ.25°.11′.11″
Distance at the perihelium .11782
Time of the perihelium 1769 October 7 10h.22m.

If now we compare these elements with those which are already known, they will approach very near to them, and they agree almost exactly with those which **Lambert** has deduced, from observations, like these, before the perihelium, but computed with much greater labour and by frequent repetitions. The inclination is somewhat too great in both cases, probably from some irregularity of the observations. **Pingre′** has computed the orbit by Laplace's method from the same observations which I have employed: but the distance and time of the perihelium, which are the only elements that he has determined, differ much more from the truth than mine: and a very superficial comparison is sufficient to shew the superior conciseness of this method.

§ 49.

The errors of the method and those of the observations being combined in this example, I shall add a second, from which the latter are excluded. The following longitudes and latitudes of the comet of 1681 are not derived from observation, but computed by **Halley**, according to his parabolic theory of this comet; so that it will appear from this instance, how accurately we may determine again, by the method here explained, the distances from the earth and sun.

Times	α	β	A	Log R
h m	s ° ″ ′	° ″ ′	s ° ″ ′	
Jan. 5. 6 1½	0 8 49 49	26 15 15	9 26 22 18	9.99282
9. 7 0	0 18 44 36	24 12 54	10 0 29 2	9.99303
13. 7 9	0 26 0 21	22 17 30	10 4 33 20	9.99325

Consequently $t'=4.0411$, $t''=4.0055$, and $T=8.0466$. Hence we find Log $M=.137562$, and the three quadratic equations

$$r'=\surd(.96754-.59292\,\varrho'+1.24328\,\varrho'^2)$$

$$r''=\surd(.96941-.40185\,\varrho'+2.20087\,\varrho'^2)$$

$$k''=\surd(.019726-.122756\,\varrho'^2+.265982\,\varrho'^2)$$

5

Now assuming $\varrho'=1$, r' becomes 1.27, $r'''=1.65$, $k''=.40$; and $T=19.75$, instead of 8.0466, that is 11.7 days too much. If we take $\varrho'='.5$, we have $r'=.99$, $r'''=1.14$, $k''=.155$, and $T=6.15$ days, or 1.9 too little. Hence we may conclude, that ϱ' cannot be very different from .56; and we find for

$\varrho'=.56$ $\varrho'=.57$
$r'=1.01262$ $r'=1.01662$
$r'''=1.19773$ $r'''=1.20641$
$k''=.18546$ $k''=.19020$
$T=8.0121$ $T=8.2402$

The error of the former supposition is —.0345, the difference of the two values of T .2281: consequently, the true curtate distance ϱ' is .56151, and we have

$r'=1.0139$ $r'''=1.1991$.

Now, according to Halley's theory, the true values were $r'=1.0144$ and $r'''=1.2000$; so that our method gives these distances perfectly correct to the third place of decimals.

§ 50.

These examples are sufficient to show the convenience and conciseness and certainty of the proposed method; upon which I shall make a few further remarks. In order to determine the time from the distances and the chord, we have the formula $T=\frac{1}{12m}\left([r'+r'''+k''']^{\frac{3}{2}}-(r'+r'''-k'')^{\frac{3}{2}}\right)$; and in order to find it the more readily, tables have been computed upon these principles; taking $B=\frac{r'+r'''+k''}{2}$, and $D=\frac{r'+r'''-k''}{2}$, we find the respective times in the tables, and their difference, or, if the angle is greater than 180°, their sum gives the value of T. Such tables are to be found in the Berlin collection, but they are not very accurate: PINGRÉ has improved and corrected them, in his *Cométographie*.

Since these tables are only calculated to hundredth parts of B and D, I have only found it convenient to employ them when no great accuracy is required, as in the first preliminary experiments with a value of ϱ'. If we wish for great precision, the proportional parts are troublesome, since the first differences alone are scarcely ever sufficient for the purpose. In this case it is much easier to compute the proper times immediately from B and D. This may be done very conveniently by the formulas Log $s' =$ Log B$+\frac{1}{2}$ log B$+1.4378117$ and Log $s'' =$ Log D $+\frac{1}{2}$ log D$+1.4378117$, the time T, in which the space in question is described, being expressed by $s' - s''$, in days. If, for instance, we take r', as in the last article, $= 1,01262$, $r''' =$ 1.19773, and $k'' = .18546$, the computation will stand thus

$$\begin{array}{rr} r' = & 1.01262 \\ +r''' = & 1.19773 \\ \hline = & 2.21035 \\ \hline \text{Half} & 1.10517 \\ \frac{1}{2}k'' & .09273 \\ \hline \text{B} = & 1.19790 \\ \text{D} = & 1.01244 \end{array}$$

Log B	.078421	Log D	.005369
$\frac{1}{2}$ Log B	.039211	$\frac{1}{2}$ Log D	.002685
Const Log	1.437812	Const Log	1.437812
Log s'	1.555444	Log s''	1.445866
s'	35.9290	s''	27.9169

Hence $s' - s'' = \text{T} = 8.0121$ days. If we require the time to seconds, we must take out the fifth place of decimals, for 1″= .0000116, and .0001 day is 8″.64.

§ 51.

In calculations of any length, it is always an advantage to have some check by which we may from time to time examine their accuracy. In the present instance we have several such expedients. At the end of the calculation it will be also of advantage to compute χ again from the elements which have been determined, and thence to find the geocentric longitude and latitude for the time of the middle observation: the one of these steps checks the calculation, at least the latter part of it, the other serves as a test of the accuracy of the elements of the orbit. I find, for instance, from the elements of the comet of 1769 determined in §§ 47, 48, for the 8 September 14h, the true anomaly 138°.19′.55″, and the logarithm of the distance from the sun 9.969155, whence the geocentric longitude is 3^{s}.10°.5′.57″, the latitude 22°.5′.52″ S. The error of longitude in $-2' 57''$, in latitude $+30''$; quantities not at all considerable for the first rough estimate.

§ 52.

We have many tables for finding the true anomaly of a comet from the time given, and the time from the anomaly, which are to be found in astronomical works and collections. But the most convenient and complete is unquestionably that which is contained in a book not very bulky and but little known, though extremely valuable, entitled, *An Account of the Discoveries concerning Comets, by* Thomas Barker, Gent. 4. Lond. 1757. Pp. 54, 1 plate. The second table in this little work gives, for every five minutes of the true anomaly, the corresponding parabolic area, and the logarithm of the distance, with the first differences, supposing the distance at the perihelium unity; and hence the true anomaly and distance from the sun may be found for every comet, and for every given time, with perfect accuracy, by a calculation which is much simpler than that which is required by the common tables. It is much to be regretted that Barker's essay was not known to Pingré. The mode of finding the orbit, described in it, is that of Newton, adapted to calculation by the author, and illustrated by a particular specification of all the triangles and proportions required in it. We find three valuable observations of the great and celebrated comet of 1744, almost six weeks earlier than those which are more commonly known, communicated to Barker by Morris; they are

			Long.			Lat. N.	
			s	°	′	°	′
1743	Oct.	22	2	26	46	7	35
		27	2	24	14	8	28
	Nov.	1	2	21	25	9	26

The hour was not recorded, but Barker supposes that it may have been about 8 or 9 in the evening, or 8h. 17m.; and he finds that these observations agree with the parabolic elements of the comet. [The table is also reprinted in *Sir Henry Englefield's* Essay *On the Determination of the Orbits of Comets,* according to *the Methods of Father Boscovich and M. de Laplace,* 4. Lond. 1793. Pp. 204, 4 plates; but without corrections: it is here presented in an improved form, having been completely re-calculated, as an appendix to Dr. OLBERS's Essay, together with a table of the anomaly in a very eccentric ellipsis, computed by a formula of LAPLACE.—Preface of the Editor, Mr. VON ZACH.]

§ 53.

Before I conclude this section, I must observe in the last place, that Mr. SCHULZE has proposed, in the *Memoirs of the Academy of Berlin* for 1782, a method which somewhat resembles mine in the principles on which it depends, and in the general progress of the calculation. But the computation is much more circuitous, and less convenient; principally because the chord of the earth's orbit is not supposed to be divided in proportion to the times, and because instead of the curtate distance from the earth, the distance of the comet from the sun in the first observation is employed as the principal unknown quantity to be determined. There is also a slight oversight with respect to the choice of observations. Mr. SCHULZE observes that LAMBERT has demonstrated that when the intervals are nearly equal, the revolving radius in the middle observation divides the chord of the comet's orbit very nearly in proportion to the times: "*pourvu qu'on emploie des observations assez distantes entr'elles.*" This might have been supposed to be an error of the press; but in applying his method to the comet of 1779, he actually selects the remotest observations which he possessed, making an interval of more than 80 days, and very naturally obtains elements of the comet from these, which differ from the truth in an unusual degree.

SECTION IV.

Correction of the Elements of an Orbit which has been determined.

§ 54.

Some correction of the method explained in the preceding section will always be required: partly on account of its own imperfection, and partly on account of the errors of observation, which must have so much the more effect on the elements as they must necessarily be taken at short intervals of time.

§ 55.

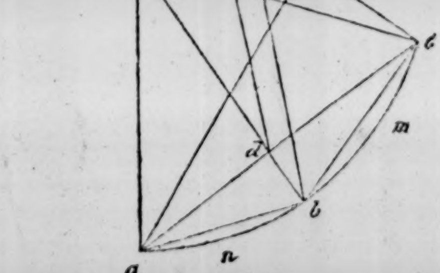
Supposing us to be in possession of a long series of observations, we may proceed immediately to employ the most distant of them for the correction of the elements, in a manner which will hereafter be explained. But if, as often happens, the comet has only been seen for two or three weeks, it will be sufficient to correct the errors depending on the mode of computation, by a process which is very easy and convenient: and in this case it will be best to begin with distances somewhat remote from each other, for instance, at an interval of 12, 14, or 16 days, especially when the apparent distance of the comet from the sun is not too small.

§ 56.

It has often been observed that our method would be mathematically correct if the revolving radii actually divided the chords of the orbits in the precise proportion of the times; for then in fact we should have correctly $\varrho'''=M\varrho'$: since, however, this is seldom exactly true, we have in reality $\varrho'''=(M+v)\varrho'+h$; and knowing the elements pretty nearly, we may now find the value of the corrections v and h.

§ 57.

For the orbit of the earth, we have $ad : dc = R' \sin(A''-A) : R''' \sin(A'''-A')$; [since $Sa=R'$ and $Sc=R'''$, and the sines of the angles at d are the same in the two triangles compared].



For the orbit of the comet, having obtained ϱ' (§ 41), we must determine the time and distance for the perihelium, and then the true anomaly ψ, at the time of the middle observation. Hence, as we have already φ and χ, the difference of the true anomalies between the first and second observation being τ, and between the first and third σ, we have $\tau = \psi - \varphi$ and $\sigma = \varphi + \chi - \psi = \chi - \tau$; and then for the chord of the orbit of the comet we have $AD : DC = r' \sin \tau : r''' \sin \sigma$: [φ being however here the true anomaly at the time of the *first* observation].

[*To be continued.*]

vi. *An easy Method of computing the Aberration of the Stars. By the* Rev. JOHN BRINKLEY, D. D., *Professor of Astronomy in the University of Dublin.*

TABLES for finding the constant Arc and Maximum of Aberration in Declination for any Star.

RULE.

$$\text{Tab. I.} \times \sin \text{Delci.} * + \text{Tab. II.} = a$$

$$\text{Tab. III.} \times \sin \text{Decli.} * = a'$$

$$\tan \text{K (or Arc for Aberr. in Decli)} = \frac{a'}{a}$$

$$(\text{K nearer O}^\circ \text{ or } 180^\circ)\ m = \frac{a}{\cos \text{K}}$$

$$(\text{K nearer } 90^\circ \text{ or } 270^\circ)\ m = \frac{a'}{\sin \text{K}}$$

Aberration in N.P.D. $= - m \cos (\text{K} - \odot$ Long.

When a is $+$ & a' $-$ K is in 2d Quadrant.
a $+$ & a' $+$ K is in 3d Quadrant.
a $-$ & a' $-$ K is in 1st Quadrant.
a $-$ & a' $+$ K is in 4th Quadrant.

N. B. When Declination of Star is S. its sine is negative.

EXAMPLE.

To find K and *m* for Capella

AR 1820 = $\overset{s}{2}$. 15° 51'
Decli.* 45° 48' N

Tab. I = —1800 log.	1.2553	Tab. III. = +4,96 log.	0.6955
Decli. * log. sin *	9.8555	Log. sin. Decli. *	9.8555
—12,90 .	1.1108	+ *a'* log.	10.5510
Tab. II. = + 5,62		— *a* log.	0.8627
a = —7,29 log.	0.8627	tan.	9.6883
Ar. Comp. cos. K	0.0463	K = $\overset{s}{11}$. 4° 0'	
m = 8,11 . .	0.9090		

For Aberration in AR in Time.

Aberration in AR = *m* cos (K ∽ ⊙ Long.)

log tan K = log tan AR + log sec. 23°. 28'

K is in the Quadrant opposite to that in which AR is ⊙

log *m* = const. log. 0.1303 + log sin AR + log sec D + log co-sec K

EXAMPLE.

AR α Cassiopeiæ = 7° 35'
Decli. = 55°. 33 N

tan. 7°. 35'		9.1243
sec. 23 28		10.0375
K = $\overset{s}{6}$. 8°. 15'.	tan.	9.1618

			s. ° '	
const. log.	0.1303	⊙ Long.	8. 5. 4.	
sin 7°. 35	9.1205'	K	6. 8. 15.	
sec. 55°. 33	10.2474'			
co sec : K	10.8432	cos	1. 26. 49	9.7382
				0.3414
m = 2, 20	0.3414			
		Aberr. in AR = +1,20		0.0796

TAB. I.

ARGUMENT.

AR. *

S.	9 . 3	10.4	11.5	S.	S.	9 . 3	10.4	11.5	S.
D.	+ −	+ −	+ −	D.	D.	+ −	+ −	+ −	D.
0	18,57	16,08	9,30	30	18	17,66	12,43	3,87	12
1	18,57	15,92	9,02	29	19	17,57	12,19	3,56	11
2	18,57	15,75	8,72	28	20	17,46	11,93	3,23	10
3	18,56	15,58	8,43	27	21	17,34	11,69	2,92	9
4	18,54	15,40	8,13	26	22	17,23	11,43	2,59	8
5	18,51	15,23	7,85	25	23	17,10	11,17	2,27	7
6	18,48	15,02	7,57	24	24	16,98	10,91	1,93	6
7	18,44	14,83	7,26	23	25	16,84	10,66	1,62	5
8	18,40	14,64	6,98	22	26	16,68	10,38	1,30	4
9	18,35	14,44	6,66	21	27	16,54	10,12	0,96	3
10	18,29	14,23	6,36	20	28	16,40	9,85	0,65	2
11	18,23	14,02	6,05	19	29	16,25	9,56	0,31	1
12	18,16	13,81	5,75	18	30	16,08	9,30	0,00	0
13	18,09	13,57	5,44	17					
14	18,01	13,35	5,12	16	D	+ −	+ −	+ −	D
15	17,94	13,14	4,80	15					
16	17,85	12,91	4,50	14	S	8.2	7.1	6.0	S
17	17,75	12,67	4,17	13					

TAB. II.

ARGUMENT.

Decli. *

D.	0	30	60	D.	0	30	60
D.	+	+	+	D.	+	+	+
0	8,06	6,98	4,02	15	7,80	5,70	2,08
1	8,06	6,92	3,90	16	7,76	5,60	1,96
2	8,06	6,84	3,78	17	7,72	5,50	1,82
3	8,06	6,76	3,66	18	7,68	5,40	1,68
4	8,04	6,68	3,54	19	7,64	5,30	1,54
5	8,04	6,60	3,40	20	7,58	5,18	1,40
6	8,02	6,52	3,28	21	7,54	5,08	1,28
7	8,00	6,44	3,16	22	7,50	4,98	1,14
8	7,98	6,36	3,02	23	7,42	4,86	0,98
9	7,96	6,28	2,90	24	7,36	4,74	0,84
10	7,94	6,18	2,76	25	7,30	4,62	0,70
11	7,92	6,10	2,64	26	7,24	4,52	0,56
12	7,90	6,02	2,48	27	7,18	4,40	0,42
13	7,88	5,92	2,36	28	7,12	4,28	0,28
14	7,84	5,82	2,22	29	7,06	4,14	0,14
				30	6,98	4,02	0,00

TAB. III.

ARGUMENT.

*AR.**

S.	0 . 6	1 . 7	2 . 8	S.
D.	+ —	+ —	+ —	D.
0	20,25	17,54	10,12	30
1	20,25	17,36	9,82	29
2	20,23	17,17	9,50	28
3	20,22	16,98	9,19	27
4	20,20	16,78	8,87	26
5	20,17	16,59	8,55	25
6	20,14	16,38	8,23	24
7	20,10	16,17	7,90	23
8	20,06	15,96	7,58	22
9	20,01	15,74	7,24	21
10	19,95	15,51	6,92	20
11	19,89	15,28	6,59	19
12	19,82	15,05	6,25	18
13	19,73	14,81	5,92	17
14	19,65	14,57	5,58	16
15	19,56	14,32	5,24	15
16	19,47	14,07	4,90	14
17	19,37	13,81	4,55	13
18	19,26	13,55	4,21	12
19	19,15	13,29	3,86	11
20	19,04	13,01	3,51	10
21	18,90	12,75	3,16	9
22	18,77	12,47	2,81	8
23	18,64	12,19	2,47	7
24	18,50	11,91	2,11	6
25	18,36	11,62	1,76	5
26	18 20	11,32	1,42	4
27	18,04	11,02	1,06	3
28	17,88	10,73	0,71	2
29	17,71	10,42	0,35	1
30	17,54	10,12	0,00	0
D.	+ —	+ —	+ —	D.
S.	11 . 5	10 4	9 . 3	S.

Art. XVI.—*The Clock's daily Rate and Error at one time of the Day being known, a Table to find its corresponding Error throughout the day.* *By* James South, Esq. F.R.S., &c.

To the person attached to the pursuit of practical astronomy, the employment of observing is by no means an ungrateful one; but it too often happens that observations, when made, require some reductions or corrections before they can be converted to any useful purpose; a circumstance which has a considerable share in causing the science to be pursued with less advantage by private individuals, than it otherwise would be. Indeed, in this respect, astronomy seems to labour under greater inconveniences than any other department of natural knowledge, unless it be that of chemistry, in which perhaps the amusement of making experiments is sometimes not a little marred by the consciousness that the washing of apparatus, *&c.*, must be the inevitable consequence.

Unable altogether to remove the drudgery of reducing observations, it behoves us, where we can, to lessen it; and, for this purpose, I beg to offer the accompanying tables shewing the error of the clock for any part of the day, its rate and error at one time of it being known; nor can I allow this opportunity to escape me, without earnestly recommending the amateur observer, to ascertain the precise state of his clock every night, previous to his quitting his observatory. He will find that a constant and uniform attention to the clock, will insensibly entail upon him, habits of accuracy in his other observations; nor do I know any one thing which distinguishes the accurate, from the slovenly astronomer so much, as the manner in which the performance of the transit-clock, is watched and registered.

⁂ The calculations upon which this table is founded, were extended to five places of decimals, so that the third place here given may be considered correct; and, for the convenience of the travelling or nautical astronomer, a table for reducing sidereal to mean solar time is subjoined, for which I stand indebted to the industry of the late Rev. Mr. Wollaston.

H.	M.	0.″10	0.″11	0.″12	0.″13	0.″14	0.″15	0.″16	0.″17	0.″18	H.	M.
0	5	.000	.000	.000	.000	.000	.001	.001	.001	.001	0	5
	10	.001	.001	.001	.001	.001	.001	.001	.001	.001		10
	20	.001	.002	.002	.002	.002	.002	.002	.002	.002		20
	30	.002	.002	.003	.003	.003	.003	.003	.004	.004		30
	40	.003	.003	.003	.004	.004	.004	.004	.005	.005		40
	50	.003	.004	.004	.004	.005	.005	.006	.006	.006		50
1	0	.004	.005	.005	.005	.006	.006	.007	.007	.008	1	0
2	0	.008	.009	.010	.011	.012	.012	.013	.014	.015	2	0
3	0	.013	.014	.015	.016	.017	.019	.020	.021	.022	3	0
4	0	.017	.018	.020	.022	.023	.025	.027	.028	.030	4	0
5	0	.021	.023	.025	.027	.029	.031	,033	.035	.037	5	0
6	0	.025	.027	.030	.033	.035	.037	.040	.042	.045	6	0
7	0	.029	.032	.035	.038	.041	.044	.047	.050	.052	7	0
8	0	.033	.037	.040	.043	.047	.050	.053	.057	.060	8	0
9	0	.038	.041	.045	.049	.052	.056	.060	.064	.068	9	0
10	0	.042	.046	.050	.054	.058	.062	.067	.071	.075	10	0
11	0	046	.050	.055	.060	.064	.069	.073	.078	.082	11	0
12	0	.050	.055	.060	.065	.070	.075	.080	.085	.090	12	0
13	0	.054	060	.065	.070	.076	.081	.087	.092	.098	13	0
14	0	.058	.064	.070	.076	.082	.087	.093	.099	.105	14	0
15	0	.063	.069	.075	.081	.087	.094	.100	.106	.112	15	0
16	0	.067	.073	.080	.087	.093	.100	.107	.113	.120	16	0
17	0	.071	.078	.085	.092	.099	.106	.113	.120	.128	17	0
18	0	.075	.082	.090	.098	.105	.112	.120	.127	.135	18	0
19	0	.079	.087	.095	.103	.111	.119	.127	.135	.143	19	0
20	0	.083	.092	.100	.108	.117	.125	.133	.142	.150	20	0
21	0	.088	.096	.105	.114	.122	.131	.140	.149	.157	21	0
22	0	.092	.101	.110	.119	.128	.137	.147	.156	.165	22	0
23	0	.096	.105	.115	.125	.134	.144	.153	.163	.173	23	0

H.	M.	0″.19	0″.20	0″.21	0″.22	0″.23	0″.24	0″.25	0″.26	0″.27	H.	M.
0	5	.001	.001	.001	.001	.001	.001	001	.001	.001	0	5
	10	.001	.001	.001	.002	.002	.002	.002	.002	.002		10
	20	.003	.003	.003	.003	.003	.003	.003	.004	.004		20
	30	.004	.004	.004	.005	.005	.005	.005	.005	.006		30
	40	.005	.006	.006	.006	.006	.007	.007	.007	.008		40
	50	.007	.007	.007	.008	.008	.008	.009	.009	.009		50
1	0	.008	.008	.009	.009	.010	.010	.010	.011	.011	1	0
2	0	.016	.017	.018	.018	.019	.020	.021	.022	.023	2	0
3	0	.024	.025	.026	.028	.029	.030	.031	.032	.034	3	0
4	0	.032	.033	.035	.037	.038	.040	.042	.043	.045	4	0
5	0	.040	.042	.044	.046	.048	.050	.052	.054	.056	5	0
6	0	.048	.050	.053	.055	.057	.060	.063	.065	.067	6	0
7	0	.055	.058	.061	.064	.067	.070	.073	.076	.079	7	0
8	0	.063	.067	.070	.073	.077	.080	.083	.087	.090	8	0
9	0	.071	.075	.079	.083	.086	.090	.094	.097	.101	9	0
10	0	.079	.083	.087	.092	.096	.100	.104	.108	.112	10	0
11	0	.087	.092	.096	.101	.105	.110	.115	.119	.124	11	0
12	0	.095	.100	.105	.110	.115	.120	.125	.130	.135	12	0
13	0	.103	.108	.114	.119	.125	.130	.135	.141	.146	13	0
14	0	.111	.117	.123	.128	.134	.140	.146	.152	.157	14	0
15	0	.119	.125	.131	.138	.144	.150	.156	.162	.169	15	0
16	0	.127	.133	.140	.147	.153	.160	.167	.173	.180	16	0
17	0	.135	.142	.149	.156	.163	.170	.177	.184	.191	17	0
18	0	.043	.150	.157	.165	.172	.180	.188	.195	.202	18	0
19	0	.150	.158	.166	.174	.182	.190	.198	.206	.214	19	0
20	0	.158	.167	.175	.183	.192	.200	.208	.217	.225	20	0
21	0	.166	.175	.184	.193	.201	.210	.219	.227	.236	21	0
22	0	.174	.183	.192	.202	.211	.220	.229	.238	.247	22	0
23	0	.182	.192	.201	.211	.220	.230	.240	.249	.259	23	0

H.	M.	0″.28	0″.29	0″.30	0″.31	0″.32	0″.33	0″.34	0″.35	0″.36	H.	M.
0	5	.001	.001	.001	.001	.001	.001	.001	.001	.001	0	5
0	10	.002	.002	.002	.002	.002	.002	.002	.002	.003	0	10
0	20	.004	.004	.004	.004	.004	.005	.005	.005	.005	0	20
0	30	.006	.006	.006	.006	.007	.007	.007	.007	.008	0	30
0	40	.008	.008	.008	.009	.009	.009	.009	.010	.010	0	40
0	50	.010	.010	.010	.011	.011	.012	.012	.012	.013	0	50
1	0	.012	.012	.013	.013	.013	.014	.014	.015	.015	1	0
2	0	.023	.024	.025	.026	.027	.028	.028	.029	.030	2	0
3	0	.035	.036	.038	.039	.040	.041	.043	.044	.045	3	0
4	0	.047	.048	.050	.052	.053	.055	.057	.058	.060	4	0
5	0	.058	.060	.063	.065	.067	.069	.071	.073	.075	5	0
6	0	.070	.072	.075	.078	.080	.082	.085	.087	.090	6	0
7	0	.082	.085	.088	.090	.093	.096	.099	.102	.105	7	0
8	0	.093	.097	.100	.103	.107	.110	.113	.117	.120	8	0
9	0	.105	.109	.112	.116	.120	.124	.128	.131	.135	9	0
10	0	.117	.121	.125	.129	.133	.137	.142	.146	.150	10	0
11	0	.128	.133	.137	.142	.147	.151	.156	.160	.165	11	0
12	0	.140	.145	.150	.155	.160	.165	.170	.175	.180	12	0
13	0	.152	.157	.163	.168	.173	.179	.184	.190	.195	13	0
14	0	.163	.169	.175	.181	.187	.192	.198	.204	.210	14	0
15	0	.175	.181	.188	.194	.200	.206	.213	.219	.225	15	0
16	0	.187	.193	.200	.207	.213	.220	.227	.233	.240	16	0
17	0	.198	.205	.212	.220	.227	.234	.241	.248	.255	17	0
18	0	.210	.217	.225	.233	.240	.247	.255	.262	.270	18	0
19	0	.222	.230	.238	.245	.253	.261	.269	.277	.285	19	0
20	0	.233	.242	.250	.258	.267	.275	.283	.292	.300	20	0
21	0	.245	.254	.263	.271	.280	.289	.298	.306	.315	21	0
22	0	.257	.266	.275	.284	.293	.302	.312	.321	.330	22	0
23	0	.268	.278	.287	.297	.307	.316	.326	.335	.345	23	0

H.	M.	0″.37	0″.38	0″.39	0″.40	0″.41	0″.42	0″.43	0″.44	0″.45	H.	M.
0	5	.001	.001	.001	.001	.001	.001	.001	.002	.002	0	5
0	10	.003	.003	.003	.003	.003	.003	.003	.003	.003	0	10
0	20	.005	.005	.005	.006	.006	.006	.006	.006	.006	0	20
0	30	.008	.008	.008	.008	.009	.009	.009	.009	.009	0	30
0	40	.010	.011	.011	.011	.011	.012	.012	.012	.012	0	40
0	50	.013	.013	.014	.014	0.14	.015	.015	.015	.016	0	50
1	0	.015	.016	.016	.017	.017	.018	.018	.018	.019	1	0
2	0	.031	.032	.033	.033	.034	.035	.036	.037	.038	2	0
3	0	.046	.047	.049	.050	.051	.053	.054	.055	.056	3	0
4	0	.062	.063	.065	.067	.068	.070	.072	.073	.075	4	0
5	0	.077	.079	.081	.083	.085	.088	.090	.092	.094	5	0
6	0	.093	.095	.098	.100	.102	.105	.108	.110	.113	6	0
7	0	.108	.111	.114	.117	.120	.122	.125	.128	.131	7	0
8	0	.123	.127	.130	.133	.137	.140	.143	.147	.150	8	0
9	0	.139	.142	.146	.150	.154	.158	.161	.165	.169	9	0
10	0	.154	.158	.163	.167	.171	.175	.179	.183	.188	10	0
11	0	.170	.174	.179	.183	.188	.193	.197	.202	.206	11	0
12	0	.185	.190	.195	.200	.205	.210	.215	.220	.225	12	0
13	0	.200	.206	.211	.217	.222	.228	.233	.238	.244	13	0
14	0	.216	.222	.228	.233	.239	.245	.251	.257	.263	14	0
15	0	.231	.237	.244	.250	.256	.262	.269	.275	.281	15	0
16	0	.247	.253	.260	.267	.273	.280	.287	.293	.300	16	0
17	0	.262	.269	.276	.283	.290	.297	.305	.312	.319	17	0
18	0	.278	.285	.293	.300	.307	.315	.323	.330	.338	18	0
19	0	.293	.301	.309	.317	.325	.333	.340	.348	.356	19	0
20	0	.308	.317	.325	.333	.342	.350	.358	.367	.375	20	0
21	0	.324	.332	.341	.350	.359	.367	.376	.385	.394	21	0
22	0	.339	.348	.358	.367	.376	.385	.394	.403	.413	22	0
23	0	.355	.364	.374	.383	.393	.403	.412	.422	.431	23	0

H.	M.	0″.46	0″.47	0″.48	0″.49	0″.50	0″.51	0″.52	0″.53	0″.54	H.	M.
0	5	.002	.002	.002	.002	.002	.002	.002	.002	.002	0	5
0	10	.003	.003	.003	.003	.003	.004	.004	.004	.004	0	10
0	20	.006	.007	.007	.007	.007	.007	.007	.007	.007	0	20
0	30	.010	.010	.010	.010	.010	.011	.011	.011	.011	0	30
0	40	.013	.013	.013	.014	.014	.014	.014	.015	.015	0	40
0	50	.016	.016	.017	.017	.017	.018	.018	.018	.019	0	50
1	0	.019	.020	.020	.020	.021	.021	.022	.022	.023	1	0
2	0	.038	.039	.040	.041	.042	.043	.043	.044	.045	2	0
3	0	.058	.059	.060	.061	.062	.064	.065	.066	.068	3	0
4	0	.077	.078	.080	.082	.083	.085	.087	.088	.090	4	0
5	0	.096	.098	.100	.102	.104	.106	.108	.110	.113	5	0
6	0	.115	.117	.120	.123	.125	.127	.130	.132	.135	6	0
7	0	.134	.137	.140	.143	.146	.149	.152	.155	.157	7	0
8	0	.153	.157	.160	.163	.167	.170	.173	.177	.180	8	0
9	0	.173	.176	.180	.184	.187	.191	.195	.199	.203	9	0
10	0	.192	.196	.200	.204	.208	.212	.217	.221	.225	10	0
11	0	.211	.215	.220	.225	.229	.234	.238	.243	.247	11	0
12	0	.230	.235	.240	.245	.250	.255	.260	.265	.270	12	0
13	0	.249	.255	.260	.265	.271	.276	.282	.287	.292	13	0
14	0	.268	.274	.280	.286	.292	.298	.303	.309	.315	14	0
15	0	.288	.294	.300	.306	.312	.319	.325	.331	.338	15	0
16	0	.307	.313	.320	.327	.333	.340	.347	.353	.360	16	0
17	0	.326	.333	.340	.347	.354	.361	.368	.375	.383	17	0
18	0	.345	.352	.360	.368	.375	.383	.390	.397	.405	18	0
19	0	.364	.372	.380	.388	.396	.404	.412	.420	.427	19	0
20	0	.383	.392	.400	.408	.417	.425	.433	.442	.450	20	0
21	0	.403	.411	.420	.429	.437	.446	.455	.464	.473	21	0
22	0	.422	.431	.440	.449	.458	.468	.477	.486	.495	22	0
23	0	.441	.450	.460	.470	.479	.489	.498	.508	.518	23	0

H.	M.	0″.55	0″.56	0″.57	0″.58	0″.59	0″.60	0″.61	0″.62	0″.63	H.	M.
0	5	.002	.002	.002	.002	.002	.002	.002	.002	.002	0	5
0	10	.004	.004	.004	.004	.004	.004	.004	.004	.004	0	10
0	20	.008	.008	.008	.008	.008	.008	.008	.009	.009	0	20
0	30	.011	.012	.012	.012	.012	.012	.013	.013	.013	0	30
0	40	.015	.016	.016	.016	.016	.017	.017	.017	.018	0	40
0	50	.019	.019	.020	.020	.021	.021	.021	.022	.022	0	50
1	0	.023	.023	.024	.024	.025	.025	.025	.026	.026	1	0
2	0	.046	.047	.048	.048	.049	.050	.051	.052	.053	2	0
3	0	.069	.070	.071	.073	.074	.075	.076	.077	.079	3	0
4	0	.092	.093	.095	.097	.098	.100	.102	.103	.105	4	0
5	0	.115	.117	.119	.121	.123	.125	.127	.129	.131	5	0
6	0	.138	.140	.143	.145	.147	.150	.153	.155	.158	6	0
7	0	.160	.163	.166	.169	.172	.175	.178	.181	.184	7	0
8	0	.183	.187	.190	.193	.197	.200	.203	.207	.210	8	0
9	0	.206	.210	.214	.218	.221	.225	.229	.232	.236	9	0
10	0	.229	.233	.238	.242	.246	.250	.254	.258	.263	10	0
11	0	.252	.257	.261	.266	.270	.275	.280	.284	.289	11	0
12	0	.275	280	.285	.290	.295	.300	.305	.310	.315	12	0
13	9	.298	.303	.309	.314	.320	.325	.330	.336	.341	13	0
14	0	.321	.327	.333	.338	.344	.350	.356	.362	.367	14	0
15	0	.344	.350	.356	.363	.369	.375	.381	.387	.394	15	0
16	0	.367	.373	.380	.387	.393	.400	.407	.413	.420	16	0
17	9	.390	.397	.404	.411	.418	.425	.432	.439	.446	17	0
18	0	.413	.420	.428	.435	.442	.450	.458	.465	.473	18	0
19	0	.435	.443	.451	.459	.467	.475	.483	.491	.499	19	0
20	0	.458	.467	.475	.483	.492	.500	.508	.517	.525	20	0
21	0	.481	.490	.499	.508	.516	.525	.534	.542	.551	21	0
22	0	.504	.513	.523	.532	.541	.550	.559	.568	.578	22	0
23	0	.527	.537	.546	.556	.565	.575	.585	.594	.604	23	0

H.	M.	0″.64	0″.65	0″.66	0″.67	0″.68	0″.69	0″.70	0″.71	0″.72	H.	M.
0	5	.002	.002	.002	.002	.002	.002	.002	.002	.003	0	5
0	10	.004	.005	.005	.005	.005	.005	.005	.005	.005	0	10
0	20	.009	.009	.009	.009	.009	.010	.010	.010	.010	0	20
0	30	.013	.014	.014	.014	.014	.014	.015	.015	.015	0	30
0	40	.018	.018	.018	.019	.019	.019	.019	.020	.020	0	40
0	50	.022	.023	.023	.023	.024	.024	.024	.025	.025	0	50
1	0	.027	.027	.028	.028	.028	.029	.029	.030	.030	1	0
2	0	.053	.054	.055	.056	.057	.058	.058	.059	.060	2	0
3	0	.080	.081	.082	.084	.085	.086	.088	.089	.090	3	0
4	0	.107	.108	.110	.112	.113	.115	.117	.118	.120	4	0
5	0	.133	.135	.138	.140	.142	.144	.146	.148	.150	5	0
6	0	.160	.162	.165	.168	.170	.173	.175	.177	.180	6	0
7	0	.187	.190	.193	.195	.198	.201	.204	.207	.210	7	0
8	0	.213	.217	.220	.223	.227	.230	.233	.237	.240	8	0
9	0	.240	.244	.248	.251	.255	.259	.263	.266	.270	9	0
10	0	.267	.271	.275	.279	.283	.288	.292	.296	.300	10	0
11	0	.293	.298	.303	.307	.312	.316	.321	.325	.330	11	0
12	0	.320	.325	.330	.335	.340	.345	.350	.355	.360	12	0
13	0	.347	.352	.358	.363	.368	.374	.379	.385	.390	13	0
14	0	.373	.379	.385	.391	.397	.403	.408	.414	.420	14	0
15	0	.400	.406	.413	.419	.425	.431	.438	.444	.450	15	0
16	0	.427	.433	.440	.447	.453	.460	.467	.473	.480	16	0
17	0	.453	.460	.468	.475	.482	.489	.496	.503	.510	17	0
18	0	.480	,487	.495	.503	.510	.518	.525	.532	.540	18	0
19	0	.507	.515	.523	.530	.538	.546	.554	.562	.570	19	0
20	0	.533	.542	.550	.558	.567	.575	.583	.592	.600	20	0
21	0	.560	.569	.578	.586	.595	.604	.613	.621	.630	21	0
22	0	.587	.596	.605	.614	.623	.633	.642	.651	.660	22	0
23	0	.613	.623	.633	.642	.652	.661	.671	.680	.690	23	0

H.	M.	0″.73	0″.74	0″.75	0″.76	0″.77	0″.78	0″.79	0″.80	0″.81	H.	M.
0	5	.003	.003	.003	.003	.003	.003	.003	.003	.003	0	5
0	10	.005	.005	.005	.005	.005	.005	.005	.006	.006	0	10
0	20	.010	.010	.010	.011	.011	.011	.011	.011	.011	0	20
0	30	.015	.015	.016	.016	.016	.016	.016	.017	.017	0	30
0	40	.020	.021	.021	.021	.021	.022	.022	.022	.022	0	40
0	50	.025	.026	.026	.026	.027	.027	.027	.028	.028	0	50
1	0	.030	.031	.031	.032	.032	.033	.033	.033	.034	1	0
2	0	.061	.062	.063	.063	.064	.065	.066	.067	.068	2	0
3	0	.091	.092	.094	.095	.096	.098	.099	.010	.101	3	0
4	0	.122	.123	.125	.127	.128	.130	.132	.133	.135	4	0
5	0	.152	.154	.156	.158	.160	.163	.165	.167	.169	5	0
6	0	.183	.185	.188	.190	.192	.195	.198	.200	.203	6	0
7	0	.213	.216	.219	.222	.225	.227	.230	.233	.236	7	0
8	0	.243	.247	.250	.253	.257	.260	.263	.267	.270	8	0
9	0	.274	.277	.281	.285	.289	.293	.296	.300	.304	9	0
10	0	.304	.308	.313	.317	.321	.325	.329	.333	.338	10	0
11	0	.335	.339	.344	.348	.353	.358	.362	.367	.371	11	0
12	0	.365	.370	.375	.380	.385	.390	.395	.400	.405	12	0
13	0	.395	.401	.406	.412	.417	.423	.428	.433	.439	13	0
14	0	.426	.432	.438	.443	.449	.455	.461	.467	.473	14	0
15	0	.456	.462	.469	.475	.481	.488	.494	.500	.506	15	0
16	0	.487	.493	.500	.507	.513	.520	.527	.533	.540	16	0
17	0	.517	.524	.531	.538	.545	.553	.560	.567	.574	17	0
18	0	.548	.555	.563	.570	.577	.585	.593	.600	.608	18	0
19	0	.578	.586	.594	.602	.610	.618	.625	.633	.641	19	0
20	0	.608	.617	.625	.633	.642	.650	.658	.667	.675	20	0
21	0	.639	.647	.656	.665	.674	.683	.691	.700	.709	21	0
22	0	.669	.678	.688	.697	.706	.715	.724	.733	.743	22	0
23	0	.700	.709	.719	.728	.738	.748	.757	.767	.776	23	0

H.	M.	0″.82	0″.83	0″.84	0″.85	0″.86	0″.87	0″.88	0″.89	0″.90	H.	M.
0	5	.003	.003	.003	.003	.003	.003	.003	.003	.003	0	5
0	10	.006	.006	.006	.006	.006	.006	.006	.006	.006	0	10
0	20	.011	.012	.012	.012	.012	.012	.012	.012	.012	0	20
0	30	.017	.017	.018	.018	.018	.018	.018	.019	.019	0	30
0	40	.023	.023	.023	.024	.024	.024	.024	.025	.025	0	40
0	50	.029	.029	.029	.030	.030	.030	.031	.031	.031	0	50
1	0	.034	.035	.035	.035	.036	.036	.037	.037	.038	1	0
2	0	.068	.069	.070	.071	.072	.073	.073	.074	.075	2	0
3	0	.103	.104	.105	.106	.107	.109	.110	.111	.112	3	0
4	0	.137	.138	.140	.142	.143	.145	.147	.148	.150	4	0
5	0	.171	.173	.175	.177	.179	.181	.183	.185	.187	5	0
6	0	.205	.207	.210	.213	.215	.218	.220	.222	.225	6	0
7	0	.239	.242	.245	.248	.251	.254	.257	.260	.262	7	0
8	0	.273	.277	.280	.283	.287	.290	.293	.297	.300	8	0
9	0	.308	.311	.315	.319	.322	.326	.330	.334	.338	9	0
10	0	.342	.346	.350	.354	.358	.363	.367	.371	.375	10	0
11	0	.376	.380	.385	.390	.394	.399	.403	.408	.412	11	0
12	0	.410	.415	.420	.425	.430	.435	.440	.445	.450	12	0
13	0	.444	.450	.455	.460	.466	.471	.477	.482	.487	13	0
14	0	.478	.484	.490	.496	.502	.507	.513	.519	.525	14	0
15	0	.513	.519	.525	.531	.537	.544	.550	.556	.562	15	0
16	0	.547	.553	.560	.567	.573	.580	.587	.593	.600	16	0
17	0	.581	.588	.595	.602	.609	.616	.623	.630	.638	17	0
18	0	.615	.622	.630	.638	.645	.652	.660	.667	.675	18	0
19	0	.649	.657	.665	.673	.681	.689	.697	.705	.713	19	0
20	0	.683	.692	.700	.708	.717	.725	.733	.742	.750	20	0
21	0	.718	.726	.735	.744	.752	.761	.770	.779	.787	21	0
22	0	.752	.761	.770	.779	.788	.798	.807	.816	.825	22	0
23	0	.786	.795	.805	.815	.824	.834	.843	.853	.862	23	0

H.	M.	0″.91	0″.92	0″.93	0″.94	0″.95	0″.96	0″.97	0″.98	0″.99	1″.00	H.	M.
0	5	.003	.003	.003	.003	.003	.003	.003	.003	.003	.003	0	5
0	10	.006	.006	.006	.007	.007	.007	.007	.007	.007	.007	0	10
0	20	.013	.013	.013	.013	.013	.013	.013	.014	.014	.014	0	20
0	30	.019	.019	.019	020	.020	.020	.020	.020	.021	.021	0	30
0	40	.025	.026	.026	.026	.026	.027	.027	.027	.028	.028	0	40
0	50	.032	.032	.032	.033	.033	.033	.034	.034	.034	.035	0	50
1	0	.038	.038	.039	.039	.040	.040	.040	.041	.041	.042	1	0
2	0	.076	.077	.078	.078	.079	.080	.081	.082	.083	.083	2	0
3	0	.114	.115	.116	.118	.119	.120	.121	.122	.124	.125	3	0
4	0	.152	.153	.155	.157	.158	.160	.162	.163	.165	.167	4	0
5	0	.190	.192	.194	.196	.198	.200	.202	.204	.206	.208	5	0
6	0	.228	.230	.233	.235	.237	.240	.243	.245	.248	.250	6	0
7	0	.265	.268	.271	.274	.277	.280	.283	.286	.289	.292	7	0
8	0	.303	.307	.310	.313	.317	.320	.323	.327	.330	.333	8	0
9	0	.341	.345	.349	.353	.356	.360	.364	.367	.371	.375	9	0
10	0	.379	.383	.388	.392	.396	.400	.404	.408	.413	.417	10	0
11	0	.417	.422	.426	.431	.435	.440	.445	.449	.454	.458	11	0
12	0	.455	.460	.465	.470	.475	.480	.485	.490	.495	.500	12	0
13	0	.493	.498	.504	.509	.515	.520	.525	.531	.536	.542	13	0
14	0	.531	.537	.543	.548	.554	.560	.566	.572	.578	.583	14	0
15	0	.569	.575	.581	.588	.594	.600	.606	.612	.619	.625	15	0
16	0	.607	.613	.620	.627	.633	.640	.647	.653	.660	.667	16	0
17	0	.645	.652	.659	.666	.673	.680	.687	.694	.701	.708	17	0
18	0	.683	.690	.698	.705	.712	.720	.728	.735	.743	.750	18	0
19	0	.720	.728	.736	.744	.752	.760	.768	.776	.784	.792	19	0
20	0	.758	.767	.775	.783	.792	.800	.808	.817	.825	.833	20	0
21	0	.796	.805	.814	.823	.831	.840	.849	.857	.866	.875	21	0
22	0	.834	.843	.853	.862	.871	.880	.889	.898	.908	.917	22	0
23	0	.872	.882	.891	.901	.910	.920	.930	.939	.949	.958	23	0

H.	M.	2″.00	3″.00	4″.00	5″.00	6″.00	7″.00	8″.00	9″.00	10″.00
0	5	0.007	0.010	0.014	0.017	0.021	0.024	0.028	0.031	0.035
0	10	0.014	0.021	0.028	0.035	0.042	0.049	0.056	0.062	0.069
0	20	0.028	0.042	0.056	0.069	0.083	0.097	0.111	0.125	0.139
0	30	0.042	0.063	0.083	0.104	0.125	0.146	0.167	0.187	0.208
0	40	0.056	0.083	0.111	0.139	0.167	0.194	0.222	0.250	0.278
0	50	0.069	0.104	0.139	0.174	0.208	0.243	0.278	0.312	0.347
1	0	0.083	0.125	0.167	0.208	0.250	0.292	0.333	0.375	0.417
2	0	0.167	0.250	0.333	0.417	0.500	0.583	0.667	0.750	0.833
3	0	0.250	0.375	0.500	0.625	0.750	0.875	1.000	1.125	1.250
4	0	0.333	0.500	0.667	0.833	1.000	1.167	1.333	1.500	1.667
5	0	0.417	0.625	0.833	1.042	1.250	1.458	1.667	1.875	2.083
6	0	0.500	0.750	1.000	1.250	1.500	1.750	2.000	2.250	2.500
7	0	0.583	0.875	1.167	1.458	1.750	2.042	2.333	2·625	2.917
8	0	0.667	1.000	1.333	1.667	2.000	2.333	2.667	3.000	3.333
9	0	0.750	1.125	1.500	1.875	2.250	2.625	3.000	3.375	3.750
10	0	0.833	1.250	1.667	2.083	2.500	2.917	3.333	3.750	4.167
11	0	0.917	1.375	1.833	2.292	2.750	3.208	3.667	4.125	4.583
12	0	1.000	1.500	2.000	2.500	3.000	3.500	4.000	4.500	5.000
13	0	1.083	1.625	2.167	2.708	3.250	3.792	4.333	4.875	5.417
14	0	1.167	1.750	2.333	2.917	3.500	4.083	4.667	5.250	5.833
15	0	1.250	1.875	2.500	3.125	3.750	4.375	5.000	5.625	6.250
16	0	1.333	2.000	2.667	3.333	4.000	4.667	5.333	6.000	6.667
17	0	1.417	2.125	2.833	3.542	4.250	4.958	5.667	6.375	7.083
18	0	1.500	2.250	3.000	3.750	4.500	5.250	6.000	6.750	7.500
19	0	1.583	2.375	3.167	3.958	4.750	5.542	6.333	7.125	7.917
20	0	1.667	2.500	3.333	4.167	5.000	5.833	6.667	7.500	8.333
21	0	1.750	2.625	3.500	4.375	5.250	6.125	7.000	7.875	8.750
22	0	1.833	2.750	3.667	4.583	5.500	6.417	7.333	8.250	9.167
23	0	1.917	2.875	3.833	4.792	5.750	6.708	7.667	8.625	9.583

A TABLE,

SHEWING THE ACCELERATION OF SIDEREAL ON MEAN SOLAR TIME.

Hours	Min.	Sec.	M.	S.	Sec.	Min.	Sec.
1	0	9.830	0	5	0.014	28	4.587
2	0	19.659	0	10	0.027	29	4.751
3	0	29.488		20	0.055	30	4.915
4	0	39.388		30	0.082	31	5.077
5	0	49.147		40	0.109	32	5.242
6	0	58.977		50	0.135	33	5.406
7	1	8.806	1	...	0.164	34	5.570
8	1	18.636	2	...	0.328	35	5.734
9	1	28.466	3	...	0.491	36	5.898
10	1	38.296	4	...	0.655	37	6.061
11	1	48.125	5	...	0.819	38	6.225
12	1	57.955	6	...	0.983	39	6.389
13	2	7.784	7	...	1.147	40	6.553
14	2	17.614	8	...	1.311	41	6.717
15	2	27.443	9	...	1.474	42	6.881
16	2	37.273	10	...	1.638	43	7.044
17	2	47.102	11	...	1.802	44	7.208
18	2	56.932	12	...	1.966	45	7.372
19	3	6.761	13	...	2.129	46	7.536
20	3	16.591	14	...	2.293	47	7.700
21	3	26.420	15	...	2.457	48	7.863
22	3	36.250	16	...	2.621	49	8.027
23	3	46.080	17	...	2.785	50	8.191
24	3	55.909	18	...	2.949	51	8.355
			19	...	3.112	52	8.519
			20	...	3.276	53	8.681
			21	...	3.440	54	8.846
			22	...	3.604	55	9.040
			23	...	3.768	56	9.174
			24	...	3.932	57	9.338
			25	...	4.099	58	9.502
			26	...	4.259	59	9.665
			27	...	4.423	60	9.830

ART. XVII. *Miscellaneous Intelligence.*

I. MECHANICAL SCIENCE.

§ THE ARTS, MANUFACTURES, &c.

1. *Application of the Air Pump.*—Mr. John Oldham of the Bank of Ireland, has recently applied the air pump to many operations in the arts, and in which substances are to be impregnated with fluids, and with great success; thus, in the sizing of paper, for instance, the paper is piled up evenly in a vessel capable of being rendered air-tight, an exhaustion is made, the size introduced, and the air's pressure admitted; when the fluid passing into the pores of the paper rises it regularly, and without injury to the fabric. In the same way paper, silk, flax, cotton, and woollen staples, either raw, spun, or woven, may be dyed very regularly. In the processes of boiling, soaking, or pickling food, &c., this process may be used to remove the air and introduce the fluids, and its application is easy and evident in numerous similar cases. An apparatus of this kind is erected at the Bank of Ireland for wetting bank-note paper preparatory to its being printed on; an immense quantity of this paper is wetted without delay, and without any injury to the paper.

2. *Adhesive Pelt.*—Mr. William Wood of Bow, Middlesex, has discovered that a light felt of hide or hair, or mixture of hide, hair, and wool, when saturated with tar is highly elastic and water-proof, and conceiving the useful application of the substance as a lining for the sheathing of ships, he manufactures it in an expeditious and economical manner, in sheets of suitable size for that purpose; such sheets being attached to the external sides and bottom of the ship, by simply nailing with copper nails, are covered with planking. The substance he terms adhesive felt; it possesses the property of elasticity in so considerable a degree, as to stretch uniformly without fracture or injury either to its texture or its complete impermeability to water, whenever the ship's seams are opened by straining in hard weather, or in more dangerous cases of the starting of planks, or the breaking of timbers as in stranding. In all such cases this material forms an impenetrable and elastic case or garment for the whole ship's bottom, and in the case of the opening of seams by straining, it recovers its first dimensions with the return of the part so opened in the release of the strain; in such cases it generally falls into the openings in a certain degree so as to render them afterwards more secure against a recurrence. He also finds it to be a complete protection against every destruction of worm in all climates; this

destructive animal is never known to penetrate the material in the slightest degree. The hair, or hair and wool, is prepared for felting by the operation of dressing or bowing, as in the practice of hat-making, and is filted in the usual manner. Sheets or portions, thus felted, are dipped into the melted tar and pitch, certain stated proportions to each other, and then undergo a slight compression to take away the extraneous or dripping quantity of the material; they are then exposed for a short time to air to dry and cool, and are considered fit for use.—*New Monthly Magazine*, July, p. 347.

3. *The Apograph.*—This is a newly-invented instrument for copying drawings upon paper, copper, or any other substance capable of receiving an impression, and upon any scale. It is said, the arts furnish no instance of an instrument resembling it either in its operation or appearance, except, perhaps, the pantograph, from which it differs in the position of the beam in an horizontal plane, and in the addition of a counterpoise to return the hand when the beam is not vertical, and in other respects. Mr. Smith, of Ayr, is the inventor.

4. *New Madder Lake.*—Mr. Field, after seven years of much labour, has prepared a lake from madder which, in point of brilliancy and strength both for oil and water-colours has, till within a short time, had nothing comparable to it in the arts, it is also of a very durable nature.—*New Monthly Mag.* Sept.

5. *Contamination of Salt for Manufactories.*—The following question having been proposed to the Academy of Sciences by the French Ministry: " What are the processes to be adopted in contaminating common salt without injury to the soda manufactories, which will not permit of its re-appropriation to the uses of common life by any secret process, or at so little expense, as to make the chances or the profits encourage fraud?"

The academy in answering say, that it is impossible to resolve the question because of the high price of salt, but that the following means will render the fraud the most difficult.

1. Colour the salt by $\frac{1}{200}$ of wood charcoal.
2. Infect it by $\frac{1}{2000}$ of oil, distilled from animal substances, or by $\frac{1}{400}$ of tar.
3. To make the mixture in the magazines.

II. CHEMICAL SCIENCE.

§ CHEMISTRY, ELECTRICITY, MAGNETISM.

1. *On the Analysis of Alkaline Minerals*, by M. Berthier.—M. Berthier, remarks in his paper on this important part of Analytical Chemistry, that the use of barytes is inconvenient from the necessity there is of repeatedly heating it with the mineral to be analyzed before the action is complete, and uncertain from the

circumstance that every time an alkaline glass is heated very highly with fixed bases in quantity sufficient to combine with the silica a portion of the alkali is volatilized, amounting in some cases, even to one half. Boracic acid complicates the analysis, and especially that part of it which relates to the alkali.

Here M. Berthier has been induced to suggest, in place of either those substances, the use of oxide of lead, and has found it so convenient in his own experiments as to have employed it constantly for more than a year at l'Ecole des Mines. At first, carbonate of lead in the proportion of three to one of the stone was used, but on fusing this in platinum or silver vessels, though a mass was obtained perfectly soluble in acids, yet the crucibles were frequently acted upon, because a portion of lead was sometimes reduced by something in the stone, or by the fumes of the fuel.

To obviate this inconvenience, nitrate of lead was used, and the following is the process: The mineral is to be pulverized and then exactly mixed with two parts of nitrate of lead and one part of ceruse also pulverized. The mixture is to be placed in a small covered platinum crucible, and this placed in another crucible also to be covered; they are then to be heated red for a quarter of an hour without being uncovered: the fusion takes place tranquilly and without swelling, and a yellow or brown transparent liquid glass is obtained. The crucible is then taken out of the furnace, and its contents poured into water, as much of it being taken out with a spatula as may be, and then the crucible itself plunged into the water. The glass splits into small portions and is readily acted on by acids. It is then boiled with nitric acid, and from time to time the portions of silica are crushed with an agate or porcelain pestle. The silica remains in a gelatinous state. The lead is precipitated by sulphuric acid, the liquor being tested ultimately by sulphuretted hydrogen that no portion may remain. The solution is then boiled with carbonate of ammonia, and the precipitate analyzed by the ordinary method. Finally, the liquid is to be evaporated to dryness, the salts calcined in a platinum crucible, the residuum collected and weighed. This residuum contains the alkali in the state of sulphate, and almost always mixed with sulphate of magnesia. It may be treated as follows:

1. Precipitate the sulphuric acid by acetate of barytes, collect the precipitate and weigh it; precipitate the barytes by carbonate or oxalate of ammonia. Evaporate the clear solution to dryness, and collect the salt left: it will be a mixture of alkaline subcarbonate and caustic magnesia; weigh it, and expose it to the air. If it contains potassa it will deliquesce; wash off the alkali from the magnesia, and weigh the latter, then ascertain whether the alkaline carbonate be of potassa, soda or lithia. When the sulphates first obtained are neutral, it is sufficient to determine very

exactly the quantity of sulphuric acid and magnesia, to learn the nature of the alkali.

Or, 2. Precipitate the magnesia and sulphuric acid by barytes water, and the excess of barytes by carbonate of ammonia; evaporate, calcine, and the alkaline subcarbonate is left pure. The magnesia may be separated from the sulphate of barytes by means of an acid.

Or, 3. Precipitate the magnesia by lime water, the lime by carbonate or oxalate of ammonia, evaporate, calcine, then the magnesia is obtained immediately, and the alkali in the state of sulphate.

In examining a mineral for alkali, in the first instance, one part of it powdered may be mixed with three parts of ceruse, the mixture put into a crucible so large as to half fill it, that crucible placed in another, and the whole heated to whiteness, until it be perfectly fluid. Remove the small crucible, and when cold, collect the glass, and those parts to which it adheres. Pulverize it and boil in common muriatic acid. When it has acted sufficiently, evaporate to dryness, and then wash with repeated small portions of boiling water. The silica will be left with most of the lead in the state of muriate. Precipitate the solution by lime water, which throws down all the other earths and oxides, and then precipitate the lime by carbonate of ammonia, boiling both together, evaporate to dryness, add a little sulphuric acid to the residue and heat it, the alkali remains as a sulphate. If there be no magnesia present, the lime water may be spared, and the precipitation at once made with carbonate of ammonia.—*Ann. de Chim.* xvii. p. 28.

2. *Compounds of Molybdenum, Chromium, Tungsten, Antimony, &c.*—The following results are from a paper on the composition of several inorganic combinations by M. Berzelius. They are to be considered as the most accurate results to be obtained by that chemist.

Molybdate of lead	Molybdic acid	39.185	-	100
	Oxide of lead	60.815	-	155.2
Molybdic acid	Molybdenum .	66.613	-	100
	Oxygen . .	33.387	-	50.12
Chromate of lead	Chromic acid	31.853	-	100
	Oxide of lead	68.147	-	213.924
Chromic acid	Chromium .	53.98	-	100
	Oxygen . .	46.02	-	85.65
Chromic oxide (green)	Chromium .	70.11	-	100
	Oxygen . .	29.89	-	42.64
Sulphuret of tungsten	Tungsten . .	74.891	-	100
	Sulphur . .	25.109	-	33.53

The blue substance which remains after the decomposition by heat of tungstate of ammonia in close vessels, is of a particular nature. It dissolves in caustic alkalies rather more slowly than the yellow acid of tungsten, and loses its colour. The solution yields the ordinary tungstates. If heated in an open cylindrical vessel, it becomes yellow at the surface, but remains blue within; a proof that its change of colour is produced by the access of air. When heated until the yellow colour was perfect, it gained in weight, but the increase was never more than 0.0025, or at the very utmost 0.003 of its weight.

I have found that tungsten forms an oxide beneath the acid. It may be produced by passing hydrogen gas over red hot tungstic acid in a tube. When the hydrogen passes without depositing vapour of water, the acid is reduced to the state of oxide. This oxide is a chocolate-coloured powder which does not in the moist way combine either with acids or bases. It takes fire at a temperature much below redness, and burns like tinder: 100 parts of the oxide produce 107 of tungstic acid, from whence it follows that the oxygen in this oxide is to that in the acid as 2 to 3.

On the much disputed and uncertain subject of the oxides of antimony, M. Berzelius after remarking on the difficulty of giving an accurate result, states that he has found two data which are easy to verify and important in their evidence: the one is, that 100 of antimony oxidated by nitric acid, and heated red, and sufficiently to decompose all antimonic acid, gives 124.8 of antimonious acid. The second equally invariable point is, that when antimonious acid is mixed with pure antimony, pulverized and heated in close vessels without access of air, the acid is converted into fusible oxide combining with a portion of the mixed metal, equal to one-third of that in the antimonious acid, proving that the ratio between the oxygen in the oxide and that in the acid is as 3 : 4. The composition is as follows :

Oxide of Antimony	Antimony	84.32	100
	Oxygen	15.68	18.6
Antimonious acid	Antimony	80.13	100
	Oxygen	19.87	24.8
Antimonic acid	Antimony	76.34	100
	Oxygen	23.66	31

The oxygen being as 3, 4, and 5.

Silica	Silicum	49.641	49.716
	Oxygen	50.359	50.284
Deutoxide of Copper	Copper	79.825	100
	Oxygen	20.175	25.272

Annales de Chim. xvii. 1.

3. *Oxide of Chromium.*—If the hydrated green oxide of chro-

nium be heated by an alcohol lamp, it will lose its water, and become of a deep green, nearly black. Let it be weighed, and then heated to redness, it will appear to inflame at the moment vividly. The cold oxide is of a fine green, has lost nothing in weight, but is insoluble in acids, or at least the loss in weight is so small as not to surpass 0.0025, and is occasioned by a little acid that adheres to it, notwithstanding its being precipitated with excess of ammonia. The acid becomes evident to the smell at the moment of ignition. This phenomenon is of a similar nature with the ignition of many metallic antimoniates, gadolinite, and the hydrate of zircon.—*Berz. An. Chim.* xvii. 12.

4. *Carburet of Nickel.*—Carburet of nickel, when obtained by heating the oxide with resin and oil, is, according to Mr. Ross, a substance very much resembling plumbago in its appearance, but is more micaceous. When nickel, reduced as above, and consequently containing carburet, was reheated to fusion, a portion of the compound rose to the surface, having the appearance of micaceous iron. It is not acted on by nitric acid, and may therefore be obtained by dissolving out the nickel by that acid.—*Annals of Phil.* ii. p. 149.

5. *On the Alloys of Chromium, Iron, and Steel.*—M. Berthier has lately been engaged on these compounds, and has given much interesting information respecting them, in a paper published in the *Annales de Chimie*, xvii. p. 55. Chromium has so strong an affinity for iron, that the presence of the latter metal very much facilitates the reduction of the former, and the combinations which result are, according to M. Berthier, more analogous to sulphurets and phosphurets than to alloys. The oxide of chrome also has so strong an affinity for the oxide of iron, as frequently to prevent its reduction, an effect that is not observed with any other substance.

Oxide of chromium heated very intensely, in a crucible lined with charcoal, was completely reduced, and gave a button that had suffered hasty fusion, was brittle, hard, grey in some places, grey-black in others, perhaps containing carbon in combination.

Mixtures of oxide of iron and oxide of chromium, in various proportions, were heated in crucibles lined with charcoal, and reduced, giving perfect combinations of the two metals. These alloys are generally hard, brittle, crystalline, of a whiter grey than iron, and very bright, less fusible, much less magnetic, and much less acted on by acids than iron, and these characters are more marked in proportion as more chromium is present. An alloy, resulting from an equal mixture of per-oxides of iron and oxide of chromium, gave a rounded button, full of cavities, lined with prismatic crystals, its fracture crystalline. Its colour whiter than platinum, and hard enough to scratch glass like a diamond. It was easily reducible to powder in a mortar, and its powder was metallic. Strong acids, and even nitro-muriatic acid, scarcely acted on it *.

Chromate of iron, being heated in a crucible lined with charcoal, the iron was only reduced into a minor state of oxidation, and acted on the magnet. Without the presence of the oxide of chrome, the iron would have been reduced.

On heating chromate of iron with an equal quantity of glass, containing 16 per cent. soda, there was reduction of part of the metals, and a loss of 10 per cent. which M. Berthier thinks is iron and chrome volatilized, because a metallic scoria appeared on the surface of the crucible †: and this loss was greater on adding borax, and increased with its quantity.

The best method of obtaining the alloy from chromate of iron, is to fuse it in a crucible lined with charcoal, with .30 of lime and .70 of silica, or with 1. of alkaline glass, or better still with .40 of borax; and to obtain as much chromium as possible, a portion of oxide of iron should be added.

In consequence of the experiments described, p. 319, Vol. ix. of this Journal, M. Berthier was induced to try the effect obtained by adding a portion of this alloy to steel. Two alloys of cast-steel and chromium were made, one with 0.01, the other with 0.015 of chromium. These both forged well, the first better than cast-steel. A knife and a razor were made from them, and both proved very good; their edges were hard and solid, but their most remarkable character was the fine damask they took, when washed over with sulphuric acid. This damask was composed of white silvery veins, and nearly resembled that given by the alloy of steel and silver. The white parts are probably pure chromium, on which acids have no action. There is room to suppose that chromic steel will be found proper for the manufacture of damask blades, which will be solid, hard, and have a fine appearance, and also for many other instruments. It was prepared by fusing together cast-steel and the alloy of chromium and iron.

6. *On the Combination of Oxides with Chlorine, Iodine, and Cyanogen, by* M. Granville.—The compounds formed by the union of oxides with chlorine, &c., have never been examined in a general manner. Messrs. Vauquelin, and Thomson have been engaged with some of them, but still a chasm exists in the chemical knowledge of substances with regard to these compounds. M. Granville has added to the number which was

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* Query, the proportion of these properties due to carbon combination with the iron ?

† May not this loss be alkali ?

known to exist, and has given the composition of many of them obtained by direct analysis.

The substance known in this country by the name of bleaching-powder, is a subchloride of lime; it consists of

Hydrate of lime,	1 atom	67.914
Chlorine	1 atom	32.086

when put into water, half the lime is liberated, and a solution of neutral chloride of lime is obtained, which consists of

Hydrate of lime,	1 atom	51.416
Chlorine	2 atoms	48.584

This solution is not altered by long boiling, for it still destroys the colour of indigo, and that even after evaporation, provided it has not been perfectly dried. Acids liberate chlorine, even the carbonic acid of the air can effect this decomposition.

The chloride of hydrated baryta is not a sub, but a neutral chloride, being formed of

2 atoms of chlorine	29.28
1 atom of hydrate baryta	70.72

Its properties are the same as those of the neutral chloride of lime.

Hydrate of zinc dissolves readily in chlorine, and the solution, when boiled for a quarter of an hour, strongly discolours indigo; evaporated to dryness, it leaves oxide of zinc, and disengages a little chlorine. On being analyzed, it was found to contain

oxide of zinc,	53.2
chlorine, 1 atom	46.8

The chlorides of magnesia and of the oxide of copper resemble that of the oxide of zinc.

Hydrated per-oxide of iron, dried between paper, when placed in chlorine gas, immediately liquefied, and gave a deep red solution that discoloured indigo. When boiled, the chlorine was disengaged, and oxide of iron deposited.

Oxides of alumium, bismuth, antimony, tin, tellurium, were not dissolved by chlorine; per-oxide of barium was reduced to protoxide, and then a chloride of that oxide formed. The protoxide of lead, nickel, cobalt, and manganese were peroxidized.

Hence chlorine appears capable of combining with most of the metallic oxides, which are not reducible by heat; of those with which it will not combine, some do not act at all on it, except at high temperatures; others peroxidize, and thus lose their tendency to combine with it, just as they lose in part their affinity for acids. Desiccation converts the chlorides of oxides, either into metallic chlorides, or chlorates; all of them, except the sub-chloride of lime, are, in their composition, analogous to the metallic chlorides, into which they may be transformed by losing their oxygen.

When red oxide of mercury is boiled with solution of chlorine, or when treated by a boiling solution of corrosive sublimate, a black crystalline matter is obtained, which is a combination of the red oxide, and the deutochloride of mercury. It is also formed by precipitating a hot solution of corrosive sublimate by potassa, not in excess. When analyzed it gave as its composition,

5 atoms of deutoxide	79.942
1 atom of deutochloride	20.058

This compound, M. Granville remarks, tends to strengthen the opinion, that the chlorides dissolve in water, without being converted into muriates; otherwise it must be admitted that decompositions and recompositions of water are effected by very weak causes, such as the affinity of chloride of mercury for oxide of mercury, or the cohesion of the oxido-chloride.

The precipitate obtained by pouring muriate of antimony into water, is an oxido-chloride of antimony. It fuses without decomposition, and is composed of

Protoxide of antimony, 7 atoms	82.01
Chloride of antimony, 1 atom	17.99

The similar preparation of bismuth is also an oxido-chloride; it has the same properties as the compound of antimony, and when analyzed gave, as its composition, 1 atom chloride of bismuth, 4 atoms oxide of bismuth.

Chlorides combine with ammonia as well as with oxides. The composition of the following three are given:—Ammonia chloride of phosphorus,

Chloride of phosphorus, 1 atom	63.502
Ammonia, 7 atoms	36.498

Ammonia chloride of tin,

Chloride of tin, 1 atom	79.06
Ammonia, 4 atoms	20.94

Ammonia chloride of mercury,

Deuto-chloride of mercury, 1 atom	94.09
Ammonia	5.91

Iodine appears to combine with oxides in the way that chlorine does. The compound with lime and strontia appear each to consist of one atom of the oxide, and one atom of iodine. Those formed by oxide of zinc, potassa, *&c.*, appeared to be perfect; but the composition is not given.

In addition to what M. Gay Lussac has said of the combination of cyanogen with oxides, M. Granville adds that the hydrate of strontian instantly absorbed much cyanogen. Hydrate of lime did the same, and became black. Hydrate of magnesia, dried in the air, also absorbed the gas. The cyanides of lime and strontian contain 1 atom of oxygen, and 2 atoms of cyanogen.

A combination of oxide of mercury and cyanide of mercury also exists; it is probably formed of

3 atoms of cyanide of mercury	1.737
2 atoms of peroxide	.992

Ann. de Chim. xvii., p. 37.

7. *On a New Salifiable Base, by* Dr. G. Brugnatelli.—Dr. Brugnatelli, in pursuing his experiments on uric acid, has discovered a new salifiable base. This substance, with his experiments on it, have been described in a memoir read to the Royal Institute at Milan, on January 4, 1821. An abridged account of it is inserted in the *Giornale de Fisica*, t. iii., p. 464.

The new substance is produced by the action of liquid acids on uric acid. Those that have been used are the sulphuric, nitric, muriatic, and acetic; and the uric acid may be either that of calculi or of birds or snakes. It is formed by adding concentrated sulphuric acid, for instance, in small quantities at a time, to uric acid, until a thick paste is formed; it will occasion swelling the liberation of gas, and a particular odour. When these signs have ceased, add water, the mass will become very white; and on standing, will separate into two parts. The solid portion is a neutral combination of the new base with sulphuric acid. The fluid is a portion of this compound dissolved in the excess of acid, and containing impurities. The sulphate is but little soluble in water, but the solution, decomposed by alkaline subcarbonates, yields a white light flocculent substance, which is the base in question. Muriatic acid is, perhaps, better than the sulphuric for the preparation of this substance, inasmuch as the muriate is more soluble. Acetic acid requires boiling to form it, and nitric acid produces it among other products at the time of its violent action.

The flocculent matter collected on a filter, appears like gelatine; in drying it contracts and splits, and when pulverized has the appearance of an earth. It has no taste or smell. It is slightly soluble in water, alcohol, acids, and alkalies. The impure acid solution is eminently distinguished by its property of giving a very fine azure precipitate with triple prussiate of potassa, and which may readily be distinguished, after a few experiments, from that caused by iron. It may, perhaps, be applicable to dyeing or painting. The neutral combination of the substance with acids does not give the blue precipitate, it requires for this purpose excess of acid.

This substance combines with various simple bodies. With iodine it forms a compound at common temperatures, of a dull yellow colour, resolved by heat into its two principles. When fused with sulphur they unite together; its compound with phosphorus is of a fine red colour, and when dissolved in water, occasions the formation of phosphuretted hydrogen, and a phosphate.

This substance has extraordinary powers of resisting heat. It might be taken for an earth, or metallic oxide, in this respect. The following are given as experimental demonstrations of its properties. An acid solution, put on a plate of zinc, gave a yellow spot with metallic splendour. This, well washed, dissolved in an acid, and tested by triple prussiate of potassa, gave a blue-white precipitate; the blue colour being attributed to the new substance. The solution that had acted on the zinc gave no blue colour with the test, but only a white.

A portion of it mixed with lamp-black and oil, and heated violently in a crucible for half an hour, left a reddish crust, the solution of which, in acids, gave an azure precipitate with the triple prussiate.

The azure matter burnt in the fire with facility, and left a residuum of a bright red colour, if the heat had been intense; but if moderate and continued, the residuum is scarcely red, and when placed in water produces flocculi of the substance and bubbles of the gas.

Ammonia dissolves the substance, making it first yellow, then green; when heated moderately, a residuum is obtained of a yellow metallic colour; if more heated it becomes white, and does not seem to differ from the substance first dissolved. The yellow matter dissolved in dilute acid gives a red tint to ferroprussiate of potassa, which exposed to the air becomes green. Other changes take place.

Nitric acid appears to alter the nature of the new substance. When it is added in a concentrated state to the substance or its salts, the prussiate does not then produce a blue precipitate, but a yellow tinge. Sulphuric acid, when assisted by heat offers similar phenomena.

One cannot help suspecting, that the blue precipitates in these experiments are occasioned by iron, and yet it is difficult to conceive how that metal, if present, should escape the observation of Dr. Brugnatelli. Further experiments are required, and of a more decisive nature, to clear up this matter.

8. *Effects of Copper on Vegetation.*—Some time since (says Mr. Phillips) I accidentally spilt some solution and oxide of copper near the root of a young poplar tree, in a short time the tree began to droop; the leaves on the lower branches dying first, and eventually those on the upper ones. On cutting a branch from the tree, I observed that the knife was covered with copper to the whole breadth of the branch, shewing that

the copper had been absorbed, and had undoubtedly proved fatal to the life of the tree.—*Ann. Phil.* ii. p. 77.

9. *On Succinic Acid, by* M. Julien.—The distillation of this acid from amber in the general way produces it in a very small quantity; by coarsely powdering the amber, and then mixing with it previous to distillation $\frac{1}{12}$ part of sulphuric acid, diluted with an equal weight of water; the succinic acid will be produced in about twice the quantity got in the old way. The acid is easily purified by crystallization from sulphurous and sulphuric acids with which it will be found contaminated. I believe the adding of sulphuric acid, in order to increase the production of succinic acid was first noticed in a late German paper; I have forgotten by whom it was there advised, that the diluted acid after being mixed with the amber should be evaporated: this makes the process tedious, and is not necessary; the distillation of the mixture without any previous evaporation will be found more advantageous.—*Annals of Phil.* ii. p. 76.

10. *Woulfe's Apparatus improved.*—The following form of Woulfe's apparatus is due to the Marquess Ridolfi. The bottles have three apertures as usual, and the middle one intended for the purpose of cleaning the bottle, or the introduction of materials, is closed either by a cork or a stopper; tubes descend by the other two into the bottles, as is usual, one a little way in the other nearly to the bottom; these tubes are small, they are fastened into the neck of the bottle and do not rise far above, perhaps three inches, before they terminate: each of these tubes is surrounded on the exterior of the bottle by a considerably larger tube as high as themselves, and fastened by cement on to the top of the necks or tubulars, so as to form a little vessel to receive mercury round the outside of the smaller tube. The connexion is then easily made between one or more of these bottles by glass tubes bent twice at right angles, and of such size as easily to slip in between the two tubes before described: when the lower ends are immersed in the mercury all is tight, and the apparatus may be set to work. This contrivance allows a little motion to the bottles without endangering them; they are instantly connected or unconnected at pleasure, and they act to a certain extent as tubes of safety.

11. *Prize Question.*—The Society of Apothecaries of Paris have offered a prize of 600 francs for—1. The best determination in what manner charcoal acts in discoloration, and what are the changes it undergoes during the action. 2. What is the influence exercised during the operation by any foreign substances which the charcoal may contain. And, 3. To establish whether the texture of animal charcoal is not one of the essential causes

of its more marked action on colouring substances. A prize of 300 francs will also be given for the best vegetable analysis, such analysis to be made on a substance used in medicine, or in the arts. The time is limited to April 1, 1822.

12. *Laws of the Propagation of Heat.*—In consequence of the investigation of M. Bellevue into the nature of meteorolites, M. Emer has been led to examine the manner in which intense heat is suddenly propagated in solid bodies. He is engaged in drawing up a memoir on the movement of heat in solid bodies, in which he considers the points of equal heat, and the points of equal motion of heat in bodies that split and break by the action of fire, and are uniform in all parts. He deduces the law by which the ruptured surfaces of these detonations and separations caused by violent dilatation are formed, and concludes—

1. That it is at protuberances, and particularly at corners and edges that this sort of mutilation should commence.
2. That the fragments should generally affect mamillary forms, or those of pyramids complete or truncated, or prisms.
3. That the bases of the pyramids, and one face of the prisms are the surfaces of the fracture, and are always convex towards their middle, and over the larger part of their surface.
4. That the other faces are often concave, as if for the most part they were the fractured surfaces of the previous fragments.
5. That in general the fracture surface of a principal fragment is its largest face, and that it is only the smaller fragments which are occasioned by the breaking up of these that present anomalies.
6. That each fragment separated is impelled by the result of the forces of dilatation, which result is perpendicular to the surface of the fracture.

M. Emer has made many useful applications of his theory to the arts and sciences.—*Journ. de Phys.* xcii. p. 158.

13. *Phosphorescence of Wounds.*—It is known that light is emitted from organized bodies, when putrefaction takes place under certain circumstances: the same phenomenon sometimes occurs in wounds, and doubtless a greater number of instances would be recorded, were they often dressed in the dark. Baron Percy who, during twenty-five years of war, has had under his care more than a million wounded, has often been deprived of the advantage of light. It was thus that he observed in a young soldier the phosphorescence of a slight wound in the leg, for more than fifteen days. In this case it might perhaps be attributed to the man's having applied compresses dipped in urine to the wound: but sometime afterwards at the siege of Manheim a vivid light, a true ignis fatuus existed, for more than six days,

over the wound of an officer, who had been dressed with compresses wetted with pure water only. Baron Percy has since had frequent opportunities of observing similar facts.

14. *On the Phosphorescence of luminous Insects.*—This subject is treated by M. Macair in a Memoir published in the *Bibl. Univers.* 1821. The insects on which his experiments were principally made, were the *Lampyrus noctiluca and splendidula*, known by the common name of glow-worms. The following are some of the general and striking results.

Solar light appears to have a constant influence on these animals. Some were put into boxes, from which the light was shut out, and when the boxes were opened in the evening, they rarely gave any light; but the same worms, in the same boxes with glass tops, and placed in the sunshine, shone brilliantly in the ensuing evening.

Heat caused these animals to become luminous, and they remained so as long as the heat was continued; it began at 22° R. was brightest at 33° R., the insect then soon died, but the light continued; at 46° it ceased. When the animals were thrown into water of 35° or 40°, they died instantly, but the light continued brilliant: at 10° higher all light was extinguished, and could not be restored. Cold, on the contrary, destroys the luminousness of the insect.

When the luminous part of the animal is cut off, the light diminishes, and in four or five minutes is gone. In a few minutes the rings move and the light re-appears, but more weakly, and then fades away. This continues two or three days, but the light is faint; if the part be warmed, then the light is brilliant, and by renewing it may be restored for two or three days together as often as is desired.

When the abdomen of a worm is opened the luminous matter is found within, formed by a particular organization. It appears as a yellowish white matter on the last three rings semi-transparent, which in the microscope appears organized, and consisting of grains confined in a ramifying fibrile structure, shining brightly in the dark. The interior surface of the ring is very transparent, but not phosphorescent. The substance is translucid, becomes opaque by drying, and then ceases to shine. It is heavier than water. Preserved openly in water it shines with a yellowish-green light for two or three hours, and then ceases shining. Heat and galvanism reproduce the light as long as the substance is not quite opaque. Up to 33° R. the light increases; at 42° it ceases, and the substance is then white and opaque like albumen. In a vacuum it ceases to shine, but reshines with air. It shines more in oxygen than in other gases. When burned it gives ammonical results. Concentrated acids extinguish the light and coagulate the substance. It is not soluble in oils. Ether and alcohol destroy the light, and coagulate the substance. Potassa dissolves the substance. It is not soluble in boiling water, but becomes more consistent in it. From the chemical character M. Macair concludes the substance to be albumen principally, and the cause of the cessation of light to be the coagulation of the albumen and its consequent opacity.

The general conclusions are—1. that a certain degree of heat is necessary to the voluntary light of glow-worms. 2. That a slightly increased heat increases the light, but much more heat destroys it. 3. That all bodies capable of coagulating albumen destroy the phosphorescence of this matter. 4. That the light does not appear except in gases containing oxygen. 5. That the pile excites it, but common electricity does not. 6. That the luminous matter is principally albumen.

15. *Relation of a remarkable electrical Phenomenon.*—The following relation is made by M. Allemand of Fleuvier Neuchatel, to M. Pictet, and is published in the *Bib. Univer.*, June 1821. M. Allemand, on the 3d of May, about ten o'clock in the evening, was caught in a violent storm of wind and rain. The thunder becoming frequent and strong, he thought it proper to close an umbrella he had with him, and hold the upper metallic point in his hand, lest it should attract the lightning. The night, dark of itself, was made more so by the great rain. Suddenly he perceived a light from above, and looking upwards found the edge of his hat luminous. Supposing at the moment the hat was on fire, he, without reflection, passed his hand over the light to extinguish it. It however only shone more strongly, a circumstance which caused some confused ideas on the nature of the light. The hand being filled with water from the hat, on shaking it, M. Allemand saw that the interior of it shone as if it were a polished metal reflecting a strong light.

Being at this time near the farm of Chaux, about ten or twelve minutes' walk from Fleurin, and fifteen or twenty from Motiers, M. Allemand, considered for a moment what he had best do, and concluded on continuing his progress. Having once filled his hand with the electrified water with impunity, he ventured to repeat the experiment, and did it fifteen or twenty times, endeavouring to ascertain whether it had odour, or produced any decrepitation or sound; but nothing of this kind could be perceived, nothing but the bright light which seemed like a brilliant varnish on the hand. The light remained for an instant only. At a few hundred paces farther on, the light on the hat still continuing, M. Allemand was surprised by the appearance of another light less bright than the former, on the smooth surface of the umbrella-handle, at the place where generally a plate of metal is placed for the name, but which plate had been removed from this umbrella. At first the finger was passed over it to extinguish it, but the phenomena were just as before, and both the rubbing and rubbed surface shone brightly. Afraid of the metal about the umbrella, it was thrown down, and M. Allemand went on his way, rubbing his hat on the sleeve of his coat; but in this way only rendering the light brighter. The thunder was more frequent than before, but still at some little distance. The crown of light continued until M. Allemand arrived near Motiers, and he attributed its cessation to the high poplar trees in the neighbourhood of that place.

Stopping at Motiers only a short time, he took a guide with a lantern to find the umbrella. Having done so he sent back the man, and went on himself towards Fleurin.

As the tempest had diminished, he used the umbrella; and as soon as the light of the lantern was sufficiently removed, he again remarked luminous appearances. These occurred at each end of the whalebone ribs, on the metal point which terminates them; the light was not so bright as the electric star, but were brilliant points like a yellow red metal, highly polished, and would, M. A. remarks, have appeared very beautiful if he had been collected enough to admire them.

M. Allemand explains these effects by supposing the atmosphere sursaturated with electricity, and that a portion of it was continually passing to the ground, through his hat, umbrella, and himself.

16. *On Voltaic Electricity*, by M. Pictet.—The following thoughts on Voltaic Electricity are by M. Pictet of Geneva.

In reflecting on this subject, we have been led to a few considerations that tend to explain the well-known fact of the great heating power, and weak chemical action of the voltaic apparatus, with few plates of large surface, compared to the apparatus of many small plates which produces little heat, but has much decomposing power.

Two very different actions may be distinguished in the voltaic apparatus. 1. The chemical action of the fluid which, in attacking the plates, disengages the electro-caloric, and in quantity greater as the action is more rapid. 2. An electro-motive force resulting from the reciprocal action of the two metals of each pair of plates, and by which the electro-caloric is put in movement in a direction according to the position of the plates.

This electro-motive force is probably exerted in a series of actions separated by very short periods of time, or time prescribed, and equal in each apparatus for a time given. The existence of these vibrations is indicated by the characteristic trembling produced by the voltaic action on the nervous and muscular system by the galvanic shock. Let us suppose 100 of these vibrations in a second. A little more or less is of no consequence.

In the voltaic apparatus, with few plates and large surface, an immense number of points in the same pair are acted on at once by the fluid; hence rapid and considerable disengagement of electro-caloric which is immediately urged forward by the electro-motive force, and conveyed by the conductors. These large plates without being numerous, ought to produce a great calorific effect, because each voltaic element or pair, produces in a tempuscule given, a quantity of electro-caloric at least proportionate to its surface, and perhaps even in a higher ratio.

It is not the same with the apparatus of many small plates. The quantity of electro-caloric disengaged in the tempuscule given, being proportionate to the surface of the plates will be inconsiderable, because of the smallness of that surface. It is urged forward by the electro-motive force at the instant of its production, and it traverses the series of elements without ever becoming considerable, because the initial disengagement was small, and cooling would diminish its quantity whilst passing the numerous metallic communications from one couple to another.

On the other hand, this apparatus will be far more effectual in producing chemical decompositions than the other, because the chemical action is probably dependant on the electro-motive action, and it increases with it, and this in a given interval of time; it is much more frequent, and consequently more productive in an apparatus with many plates than in one with few: thus, for instance, suppose 100 of the electro-motive and decomposing vibrations in a second of time, an apparatus with six pair of large plates would only produce 600 of these active vibrations in a second, whilst an apparatus with 500 pair of small plates would produce 500 × 100 = 50,000 similar vibrations in the same time.

It may be also that the extreme abundance, and so to speak, violence of the electro-caloric current in the apparatus with large plates, injures its chemical effect by the mechanical impulsion which the current exerts on its entrance into the fluid to be decomposed; whilst in the many small plated apparatus, the current of electro-caloric being less powerful, and the vibrations more numerous, the polar decomposing effect is more energetic.

These *thoughts*, M. Pictet judiciously adds, are merely given as *conjectures!* they wait the result of experiments, to be either confirmed or rejected.—*Bib. Univer.* xvi. p. 293.

17. *Electro-Magnetic Instruments invented by* M. de la Rive.—Two excellent little instruments have been invented by M. de la Rive, of Geneva, to illustrate the various phenomena of electro-magnetism, with very little trouble, and at a very trifling expense. The first consists of a small voltaic combination attached to a cork; the plate of zinc is nearly half an inch wide, and extends about an inch and a half below the cork, its

upper end passing through the cork to the upper surface; the slip of copper is the same width as the zinc, but passes round the zinc, being opposed to both its surfaces as in Dr. Wollaston's construction, its upper end also appears through the cork. A piece of copper wire, covered with silk thread, is coiled five or six times, and tied together, so as to form a ring about an inch in diameter, and the ends of the wire are connected, one with the zinc, and the other with copper slip above the cork. When this small apparatus is placed in water slightly acidulated by sulphuric acid, the voltaic apparatus is active enough to make the ring highly magnetic; and by presenting a magnet to it in different directions, it may be attracted or repelled, and presents all the phenomena of a mobile conducting wire.

The other apparatus is also a little voltaic combination hung from a cork, but the plates are connected together, not by a ring of wire, but by a helix. The helix is made of similar wire to the ring, it is about one-third of an inch in diameter, and the two ends of the wire are returned through the helix till near the middle, when they are made to pass to the outside between the spirals, then being connected with the upper ends of the plates, the helix lies on the cork, with its two ends equally distant from the centre, the course of the electricity being along the wire, from one end of the helix to the other, and then back to the plates. When placed on acidulated water the helix becomes magnetic, and its extremities become opposite magnetic poles, being attracted and repelled by the poles of a magnet, just as another magnet would be in the same situation.—*Bib. Univer.* xvi. p. 201.

These little instruments took their rise from the floating needle of M. Naef. This was composed of a strip of silver and one of zinc connected together, bent, and floated on cork. These, when placed on acid, were attracted and repelled by the magnetic pole.—*Bib. Univer.* xvi. p. 120.

18. *Contact in Voltaic Electricity.*—In making experiments in voltaic electricity and electro-magnetism, where numerous repetitions of contact between wires are required, it is extremely useful, if these wires are copper, to rub the ends over with a little nitrate of mercury; an amalgam is thus formed on the surface of the copper, which does not oxidate or become dirty as copper itself does, but remains bright, and fit for voltaic contact for a long time. M. F.

19. *Magnetism by Electricity.*—M. Van Bech has remarked, that a very slight degree of common electricity is sufficient to produce magnetism in a needle. A helix was made, and an unmagnetized needle placed in its interior; then one end being held in the hand, the other was brought near the conductor of

an electrical machine, and sparks passed to it. After a number of these had been taken, the needle, on examination, proved to be magnetic.—*Bib. Univer.* xvii. p. 23.

20. *New Electro-Magnetic Apparatus.*—Since the paper in the preceding pages has been printed, I have had an apparatus made by Mr. Newman, of Lisle-street, for the revolutions of the wire round the pole, and a pole round the wire. When Hare's calorimeter was used to connect with it, the wire revolved so rapidly round the pole, that the eye could scarcely follow the motion, and a single galvanic trough, containing ten pair of plates, on Dr. Wollaston's construction, had power enough to move the wire and the pole with considerable rapidity. It consists of a stand, about three inches by six, from one end of which a brass pillar rises about six inches high, and is then continued horizontally by a copper rod over the stand; at the other end of the stand a copper-plate is fixed with a wire for communication, brought out to one side; in the middle is a similar plate and wire; these are both fixed. A small shallow glass cup, supported on a hollow foot of glass, has a plate of metal cemented to the bottom, so as to close the aperture, and form a connexion with the plate on the stand; the hollow foot is a socket, into which a small cylindrical bar magnet can be placed, so that the upper pole shall be a little above the edge of the glass; mercury is then poured in until the glass is nearly full; a rod of metal descends from the horizontal arm perpendicularly over this cup; a little cavity is hollowed at the end and amalgamated, and a piece of stiff copper wire is also amalgamated, and placed in it as described in the paper, except that it is attached by a piece of thread in the manner of a ligament, passing from the end of the wire to the inner surface of the cup; the lower end of the wire is amalgamated, and furnished with a small roller, which dips so as to be under the surface of the mercury in the cup beneath it.

The other plate on the stand has also its cup, which is nearly cylindrical, a metal pin passes through the bottom of it, to connect by contact with the plate below, and to the inner end of the pin a small round bar magnet is attached at one pole by thread, so as to allow the other to be above the surface of the mercury when the cup is filled, and have freedom of motion there; a thick wire passes from the rod above down perpendicularly, so as to dip a little way into the mercury of the cup; it forms the connecting wire, and the pole can move in any direction round it. When the connexions are made with the pillar, and either of the wires from the stand plates, the revolution of the wire, or pole above, takes place; or if the wires be connected with the two coming from the plates, motion takes place in both cups at once, and in accordance with the law stated in the paper. This apparatus may be much reduced in size, and made very much more delicate and sensible.

M. F.

III. NATURAL HISTORY.

§ MEDICINE, &c.

1. *On the Use of Iodine in Medicine, by* Dr. Coindet.—Since the first discovery of the use of this substance, in cases of bronchocele, particular notice has been taken in this Journal of the results of its application, and the conclusions to be drawn from them. See x. 191, and xi. 407. In another memoir, published by Dr. Coindet, and of which the principal points will be condensed below, a new, and in many cases better mode, of administering it is pointed out, and its good effect in schrofulous cases detailed, with the advantages which will probably result from its use in this disease.

It appears that many of the dangerous symptoms caused by iodine, may be attributed, some to its local action upon the mucous membrane of the stomach, which with some persons, cannot bear the remedy uninterruptedly administered, or in increased doses, with impunity; and others to its particular action on the lymphatic system.

The symptoms belonging to these two actions differ essentially. The second action is that which, when properly directed, gives the remedy its usefulness. In order to avoid the first, Dr. Coindet endeavoured to introduce it into the system by other ways than the stomach. An ointment was made, of 1½ ounce of hogs'-lard, and half a drachm of hydriodate of potassa. A piece of the size of a nut was prescribed night and morning, to be rubbed in on the goître, or the enlarged scrophulous glands, or those of the neck. In the first case of its administration in this way, the tumour softened after eight days of friction; in fifteen days the diminution was much greater, and the tumour had divided into many small lobes, distinct one from another; at the end of a month it had entirely disappeared, without any other effect being produced by the iodine.

After that twenty-two persons were treated in the same way. Above half the number were cured in between four and six weeks, and the others to a greater or less degree.

Iodine, thus introduced into the absorbent system, presents the same results as when administered internally. The duration of treatment, the thickening and softening of the skin, the softening of the goître, its diminution and separation into many cists, tumours, or lobes, the cessation of oppressed respiration, and alteration of voice, are exactly the same.

In most cases this mode of treatment sufficed alone to remove the tumour; when it did not, a small quantity administered internally completed the cure. In one case, when the remedy had been applied by the stomach, but only with partial success, external application of it almost entirely removed the goître.

Though none of the dangerous symptoms, sometimes attending the former mode of administration had been observed accompanying the latter treatment, yet Dr. Coindet took all his usual precautions, and when the action on the goître was well established, suspended the application of iodine for eight or ten days; it was then resumed, and thus any evil resulting from a saturation of the system with iodine, or a too rapid action on the lymphatics avoided.

In all these cases the utmost attention was paid to the local treatment of the goître, inasmuch as it contributes powerfully towards the cure. It consists principally in the application of leeches, and sometimes emollient fomentations. The leeches frequently removed pain, and sometimes when iodine alone would not act on the goître, the application of leeches decided the cure. It is not sufficient, therefore, in cases of the goître, scrophula, or other disturbed states of the lymphatic system, to prescribe iodine in a careless or common way, but the other indications must be attended to, to ensure the success to be expected from so powerful a remedy.

From the power of this remedy on the absorbent system, Dr. Coindet was induced to try its effects in cases of scrophula without fever, or indolent swellings of the glands of the neck, and with great success. It was given in solution, in smaller doses than for the goître, and combined with bitters or some aromatic syrup. The course of the cure was the same as for the goître. The tumours became loose, mobile, small, and soft, but the enlargement of the cellular structure feeling like an empty cist, was sometime before it entirely disappeared. In two cases mentioned the cures were decided and rapid.

The application of the iodine externally in these cases was almost as effectual as internally, but the latter mode was generally preferred, because of the tonic effect of the remedy in small doses. Sometimes iodine would have no effect at all, a result which occurs now and then with the goître.

Dr. Coindet, led by the powerful action of this remedy on the absorbent system, then extends his views to some further applications of it. The use of it alternately with mercury, or the iodide of mercury, is proposed in complicated cases of scrophula and syphilis, and in those cases where the syphilitic virus of the parent causes the devolopement of scrophula in the child. From the analogy existing between diseased ovaries, and the affections of the thyroid glands, the probable use in the former cases is pointed out, and as in some goîtres which appeared to be serous cists, a cure was effected, it is also suggested that iodine may be useful in cases of dropsy.

In proposing these various views of the probable use of iodine, Dr. Coindet disclaims any idea of making it an universal medicine. He remarks that all these diseases have something in common, and that the action of the remedy on the absorbent system, is the principle on which he founds his hope of its utility in them.—*Bib. Univer.* xvi. p. 320.

2. *Use of Nitrate of Silver in Medicine.*—The *Giornale di Fisica*, tom. xi., contains at p. 355, a paper by Il C. Sementini, on the use of nitrate of silver in cases of epilepsy. After remarking on the difficulty which occurs in treating such cases, and the good effects that have been observed in using the nitrate of silver, and its superiority in this respect over all other remedies, both as to the effect it produces, and the little inconvenience it causes; the Cavalier states, that to secure the good effects belonging to it, the nitrate of silver should be well triturated with the vegetable extract, in combination with which it is given; that the first doses should be small, and the quantity gradually increased to six or eight grains, or even more, in a day: that the use should not be continued very long together; and that the patient should keep out of the action of light. The latter precaution is necessary, to prevent the discoloration of the skin, which sometimes happens after a long and copious use of this remedy. The precaution, however, only regards avoiding the meridian sun-light.

It frequently happens, in the use of this medicine, that a species of cutaneous eruption, consisting of small pustules, occurs. This may be regarded as a certain proof of the good effects of the medicine.

In the early part of this paper, Il C. Sementini, in endeavouring to remove the impression existing against nitrate of silver, because of its poisonous qualities, remarks, that being mixed with vegetable extract, it is not really the salt, but the oxide, that is given; and, therefore, the observations of M. Orfila, on the nitrate as a poison, have nothing to do with the power of the remedy. At the same time, as an argument for using the nitrate in place of the oxide, it is remarked, that at the moment of decomposition a combination is, probably, effected between the extract and the oxide; and that actually the salt is found most efficacious.

Being assured of the use of nitrate of silver in epileptic affections, and reasoning upon its tonic effect, Il C. Sementini was induced to try its powers as a remedy in cases of paralysis. The first instance quoted is of a gilder, who, probably from the fumes of mercury, had become very paralytic. An eighth of a grain of nitrate of silver was prescribed at first, but the dose was increased every other day; by the time that three grains were taken the good effects were evident, and in twenty days more the man was perfectly restored. In another instance every part of the body and limbs were paralyzed but the head. A small quantity was given at first, but it was increased to eight grains per day, and it effected a cure.

Three other instances are then adduced, in all of which cures were effected: and the Cavalier expresses his hopes that in the hands of other medical men, it will be found as effective and as important as in his own.

3. *Use of Chlorine in Hydrophobia.*—In a book published by Doctor Previtali, of which notice is taken in the *Giornale di Fisica*, tom. iii., p. 357, an extended account is given of the successful administration of chlorine in cases of hydrophobia. Several persons were bitten by a dog, some of these died with all the symptoms of hydrophobia, others were treated with the chlorine, and though the symptoms returned once or twice on the early cessation of the remedy, yet they were vanquished by its continuance, and a perfect cure effected. The medicine consists of four scruples of saturated solution of chlorine, with four ounces of aromatic water and half an ounce of syrup of lemons, to be taken daily. The cases seem perfectly authenticated, for the authorities being earnest to obtain all possible knowledge upon the subject, a correspondence took place in consequence, which is published, and contains the progress of the cures. The symptoms which occurred when the remedy was intermitted; and the immediate effect of the remedy, on administration, were of the most decided nature.

Some account is given in the *Gazetta di Milano* of September 13, 1820, of the application of solution of chlorine as a remedy in cases of spotted fever, by Dr. Sacco, of Milan. Reasoning from its effect on infectious matter, and finding that two, or even three, ounces of the solution might be drank at once without injury, he administered it in cases of spotted fever, and found that in two or three days the effect was to reduce the disease to a simple fever, to shorten the period of its duration, and to lessen the diminution of strength, and other bad consequences, that remain after the fever is removed. And from the constant good effect produced in these, and similar cases, Dr. Sacco states his conviction that it will be of the utmost service in the putrid fever, yellow fever, plague, and all other contagious disorders.

The dose prescribed by Dr. Sacco is one ounce of the solution in three ounces of water, repeated four times a day; a spoonful of honey being taken after each dose. He also directs that the body should be washed three times a day with the same solution.

4. *Medical Prize Question.*—The Academic Society of Nantes

have proposed a prize of 300 francs for the best answer to the queries, "What are the origin, the causes, and the nature, of the yellow fever?" "What are the means to prevent it?"

§ II. MINERALOGY, GEOLOGY, METEOROLOGY, &c.

1. *New Mineral Substance.*—Mr. J. Deuchar found, a few weeks ago, a new mineral substance imbedded in limestone. It melts at a candle, and burns on a wick, or on paper. In the cold it is insoluble in alcohol, potash, or oil of turpentine, nor is it acted upon in the cold after five days' exposure to sulphuric, muriatic, or nitric acids.—*Annals of Philosophy*, ii. p. 236.

2. *Remains of Art in a Limestone Formation.*—The following geological fact is stated by Professor Silliman, as translated from Count Bournon's *Mineralogy:*

During the years 1786, 7, and 8, they were occupied near Aix, in Provence, in France, in quarrying stone for the rebuilding, upon a vast scale, of the Palace of Justice. The stone was a deep grey limestone, and of that kind which are tender when they come out of the quarry, but harden by exposure to the air. The strata were separated from one another by a bed of sand, mixed with clay, more or less calcareous. The first which were wrought presented no appearances of any foreign bodies, but after the workmen had removed the first ten beds, they were astonished when, taking away the eleventh, to find its inferior surface, at the depth of forty or fifty feet, covered with shells. The stone of this bed having been removed, as they were taking away the sand which separated the eleventh bed from the twelfth, they found stumps of columns and fragments of stones half wrought, and the stone was exactly similar to that of the quarry. They found moreover coins, handles of hammers, and other tools, or fragments of tools, of wood. But that which principally commanded their attention was a board, about an inch thick, and seven or eight feet long; it was broken into many pieces, of which none were missing, and it was possible to join them again one to another, and to restore its original form, which was that of the boards of the same kind used by the masons and quarrymen; it was worn in the same manner, rounded, and waving on the edges.

The stones, which were completely or partly wrought, had not at all changed in their nature, but the fragments of the board and the instruments, and the pieces of instruments of wood, had been changed into agates, which were very fine. and agreeably coloured. Here then (observes Count Bournon) we have the traces of a work executed by the hand of man, placed at the depth of fifty feet, and covered with eleven beds of compact limestone; every thing tended to prove that this work had been executed upon the spot where the traces existed. The presence of man had then preceded the formation of this stone, and that very considerably, since he was already at such a degree of civilization that the arts were known to him, and that he wrought the stone, and formed columns out of it.

3. *On Meteorolites, by* M. Fleuriau de Bellevue.—A paper, by M. Fleuriau de Bellevue, was read to the Academy of Sciences last year, on meteoric stones, and particularly on those which fell near Jonzac, in the department of Charente. This paper is long, and contains much minute investigation of those appearances, which, accompanying these phenomena, afford the only means of ascertaining their real nature. We shall, probably, take an opportunity of abstracting and condensing this paper; in the mean time the following conclusions are presented as those drawn by M. Bellevue.

1. The appearances presented by the crust of meteorolites seem to prove that their surface has been fused whilst rapidly traversing the flame of the meteor, and rapidly solidified into a vitreous state on leaving that flame.

2. They prove that in the first moments the movement of the meteorolites was simple, that is, that they did not turn round on their own axis whilst those two effects took place.

3. That the impulse each meteorolite has received has almost always been perpendicular to its largest face.

4. That the largest face is almost always more or less convex.

5. Our meteorolites (those of Jonzac) offer new proofs of the pre-existence of a solid nucleus to bolides or meteors.

6. This nucleus could not contain the combustible matter which produces the inflammation of the meteor.

7. It cannot have suffered fusion during the appearance of the phenomena.

8. The gaseous matter which surrounds this nucleus is dissipated without producing any solid residuum. No trace of this matter appears ever to exist in the crust of the meteorolites.

9. Meteorolites are fragments of those nuclei which have not been altered in their nature, but simply vitrified at their surfaces.

10. Many of the irregular forms which these fragments present may be referred to determinate geometric forms.

11. These latter forms are the consequence of the rapid action of a violent fire, according to a law of the movement of heat in solid bodies, discovered by M. Emer.—*Journal de Physique*, xcii. p. 159.

4. *Skull found in a Tree.*—A very precise description, ac-

companied by wood-cuts, is given in the *Monthly Magazine* for September, p. 102, of a skull found in a tree. The tree was an ash, and was cut down about two years since in Penley Abbey Farm, near Warwick. It was supposed to be about eighty years old. The skull was closely imbedded in the solid part of the trunk, about nine feet from the ground, and was discovered when the tree was sawn up into rafters. The piece in which the skull lay being cut out a section of it has been made. The wood around the bone was every where perfectly sound, except in one small place, where it is decayed, but this had no communication with the exterior at the time the tree was cut. The grain of the wood was completely deranged, and seemed to embrace the bone round which it had found. The skull is supposed to have belonged to a deer. It is now in the possession of the Rev. Thomas Cottle, of Warwick Borough.

IV. GENERAL LITERATURE.

1. *Ancient MSS.*—M. Maio still continues to be successful in his search after lost works of ancient writers; he has lately found several parts of the books of Polybius, Diodorus, Dion Cassius, some fragments of Aristotle of Ephorus, of Timeus, of Hyperides, of Demetrius, of Phalaris, &c., some parts of the unknown writings of Eunapius, Menander of Byzantium, Prescius, and of Peter the Protector. They were discovered in a MS. containing the harangues of the rhetorician Aristides, from a large collection of ancient writings made by order of Constantius Porphyrogentes, of which only a small part are known to be extant; the writing appears to be of the eleventh century. M. Maio has also found writings of the Greek and Latin fathers prior to St. Jerome, with other valuable works.

2. *Statuary Marble.*—Some remarkably fine statuary and other marble quarries have lately been discovered at Scravazza, in Tuscany, much superior to any thing of the kind at Coanara, which threaten to rival and lower the pride of the latter-mentioned place. His Royal Highness the Grand Duke of Tuscany gives great encouragement and protection both to commerce and the fine arts within his dominions.

3. *Inventor of the Steam Engine.*—A letter was published in the *Gentleman's Magazine* for 1811, which Mr. Tilloch has again brought into notice. The writer of it refers to the Harleian MSS. for proof that the real inventor of the Steam Engine was Samuel Morland, master of the works to Charles II. Morland wrote a book upon the subject, in which he not only shewed the practicability of his plan, but even calculated the power of different

cylinders. The book is extant in the above collection. It was presented to the French king in 1683, and copies were then shewn at St. Germains. The author dates his invention in 1682, seventeen years prior to Savary's patent. The description of the MS., in which Morland explains his invention, will be found in the improved Harleian Catalogue, Vol. iii., No. 5771, and it is also pointed out in the preface to that volume, Sect. 22.

4. *Medico-Chirurgical Society of Edinburgh.*—It is with pleasure that we announce the formation of a Medico-Chirurgical Society in Edinburgh. The Society is formed upon the model of the Medico-Chirurgical Society of London, and has in view precisely similar objects. Most of medical professors in the university, and many of the most respectable practitioners in the city, have co-operated in its formation. Dr. Duncan, senior, has been elected its first president. Its sittings commence in the approaching winter session.

In addition to ordinary and honorary members, provision is made for the admission of corresponding members; and it is hoped that many, in almost every part of the world, and such especially as retain a grateful recollection of the advantages they derived from their alma-mater, will not be backward in supplying interesting communications.

Communications may be transmitted to the president of the Society, or to either of the secretaries, according to the following addresses:

Dr. W. P. ALISON, 44, Heriot-row, Edinburgh.
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